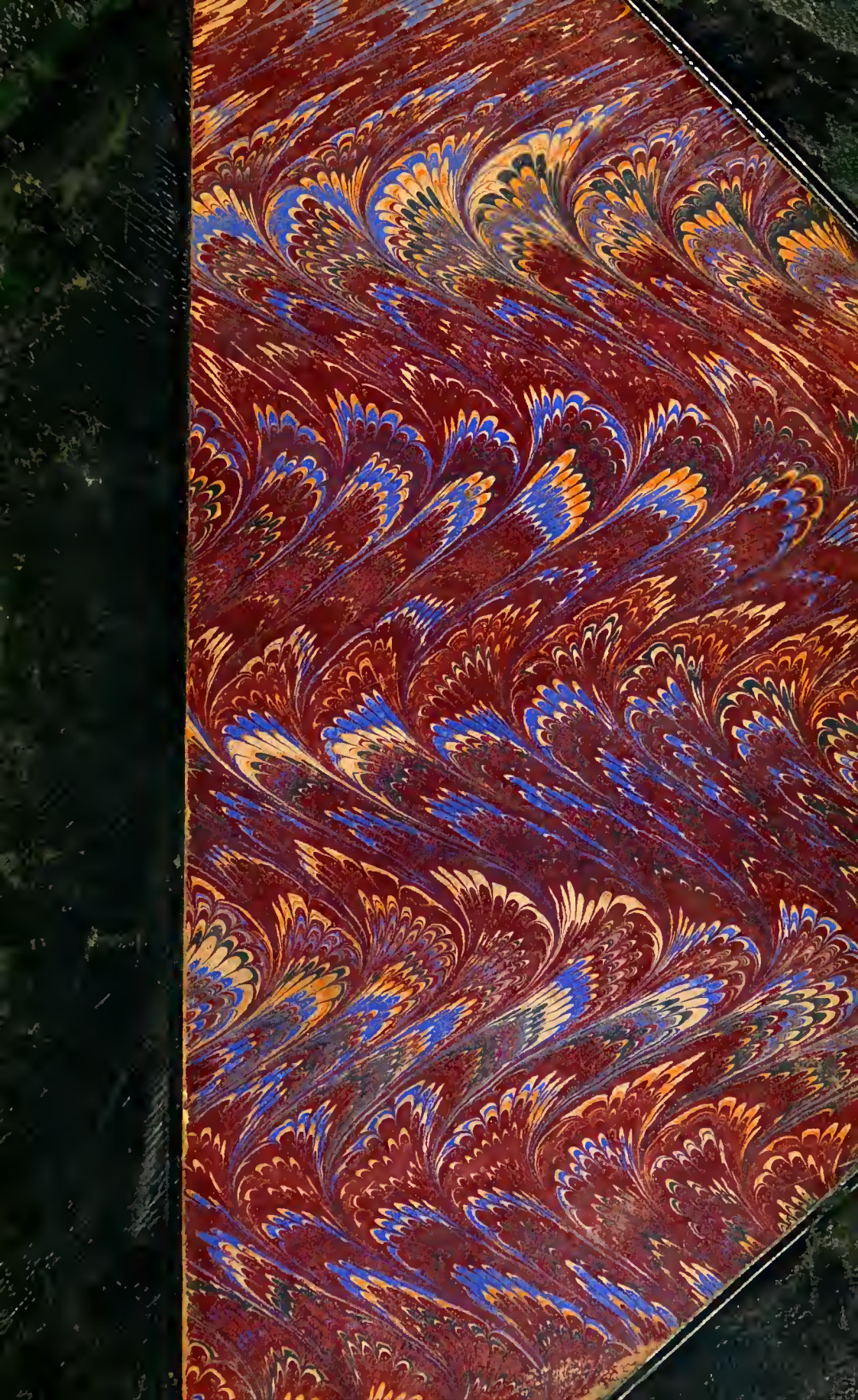


ODIUM MEDICUM

AND

HOMŒOPATHY.

"The Times" Correspondence,

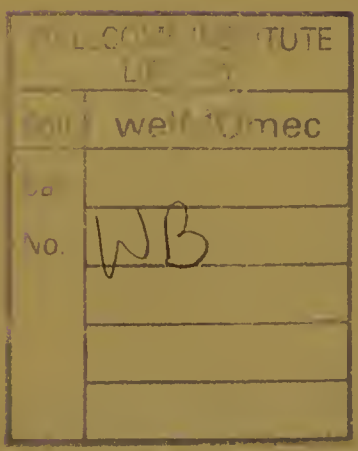
Reprinted by permission of the Proprietors of "THE TIMES."

EDITED BY

JOHN H. CLARKE, M.D.,

Editor of the HOMŒOPATHIC WORLD, &c.

LONDON:
THE HOMŒOPATHIC PUBLISHING COMPANY,
12, Warwick Lane, Paternoster Row, E.C.



CONTENTS.

PREFACE.

In view of the great interest taken by the public in the correspondence on "*Odium Medicum and Homœopathy*" that has recently occupied so much space in *The Times*, and considering how completely the subject was discussed, the publishers of The Homœopathic World have deemed it desirable to reprint the letters entire. They have obtained the consent of the proprietors of *The Times*, and, as far as possible, have submitted the proof of the letters to their authors. They are thus in a position to present to the public in handy form an authorised and authentic account of the most complete threshing out of the question of homœopathy that has ever seen the light of the public press, and a most vivid picture of the attitude of the two schools of medicine towards each other.

Prefixed to the letters themselves will be found an account of the trial out of which the discussion arose. A surgeon, Mr. Kenneth Millican, who is a member of the old school, had become the victim of persecution because he was willing to tolerate members of the new school. He sought to defend himself in the law courts, and in the first hearing of the case he was successful. In the appeal, where the merits of the case were not gone into, but only the legal points of it, the judgment of the lower court was reversed. But before this had happened the case had been virtually taken out of the law courts to the bar of public opinion by Lord Grimthorpe's letters to *The Times*.

Lord Grimthorpe's letters were answered by certain members of the allopathic school, who, whilst denying the imputation of *odium*, approved of and attempted to justify the action of Mr. Millican's intolerant colleagues. But besides attempting to justify intolerance they made an attack on homœopathy and its professors. This attack brought to the front many champions, both medical and lay, to defend homœopathy, whilst Mr. Millican maintained his original ground and pleaded for tolerance. To these the allopathists again replied, and the battle soon became general. The extent of ground covered by the conflict, and the number of the combatants, may be seen by the following letters. The length to which it might have been carried may be judged by the statement of the editor of *The Times* that he has published only a portion of the letters he received. The discussion would apparently never have ceased if the editor had not judiciously put a stop to the shedding of ink, when it had reached as clear a point of finality as these discussions ever do.

In my capacity as editor of the correspondence I have little to do. It is not for me to say who had the best or who the worst of the discussion: the letters stand to answer for themselves; and the article by the editor of *The Times* saves me the necessity of any lengthy summing up, and fitly closes the whole correspondence. In an "Appendix" we publish the last letter written by Lord Grimthorpe, and not inserted in *The Times*, and one or two other letters bearing on the discussion. Also I have added one or two foot-notes explanatory of certain points.

It would have occupied too much space in this volume to have quoted the opinions of the public press on the merits of the case; but these may be found collected in the February and March numbers of The Homœopathic World, along with other matters of interest connected with the discussion.

JOHN H. CLARKE.

34, *Harrington-road, S.W.*,
February, 1888.

ODIUM MEDICUM AND HOMŒOPATHY.

THE ACTION
WHICH GAVE RISE TO THE CORRESPONDENCE:
"TIMES" REPORT.

MILLICAN V. SULIVAN AND OTHERS.

The hearing of the case before Mr. Justice Manisty and a Special Jury took place on December 14, 1887, and was reported as follows in *The Times*. The chief facts are contained in the judgment.

Mr. Lockwood, Q.C., and Mr. H. F. Dickens were for the plaintiff; Sir H. James, Q.C., and Mr. Edward Pollock were for the defendants.

His Lordship, in giving judgment, said the action was brought by the plaintiff against ten persons said to be members of the Committee of Management of the Queen's Jubilee Hospital, Gloucester-terrace. The action was dismissed against one; it was therefore in reality against nine. The plaintiff complained that these nine did wrongfully and improperly suspend him from his office as one of the medical men who constituted the medical staff. The hospital was constituted in the present year. By rule 6, the staff was to consist of a president, vice-president, patrons, patronesses, governors, trustees, treasurer, and a committee of management, a secretary, a ladies' committee and a ladies' secretary thereto, an efficient medical and surgical staff, a resident matron, and an efficient staff of male and female nurses and other servants as might be deemed necessary by the committee for the economical working of the hospital. By rule 7, the government of the hospital was to be conducted by a committee, consisting of an equal number of the medical and surgical staff and lay members, all of whom were to be governors of the hospital; one-third of the lay members were to retire annually by rotation, but were eligible for re-election at the annual general meeting. It seemed that the committee really consisted of eleven; how then they could have an equal number of the medical and lay members he did not know, but it did not affect his judgment. By rule 8, the committee of management were to appoint at any time, subject to confirmation by show of

hands, or ballot if required, at the annual or any special general meeting of the governors, as many physicians and surgeons as might be desirable for the effieient eare of the patients, the appointment to be for one year and to be subjeet to eonfirmation by the governors at the next annual general or speeial meeting whieh should be held subsequent to such appointment. It seemed, therefore, that the eommittee had power to appoint the physicians and surgeons, and the plaintiff was one so appointed. The question is whether the committee had power to suspend the plaintiff. It was raised in the paragraph of defence, whieh said, "The defendants, in pursuanee of the printed rules and regulations of the said hospital, held a meeting on May 26, 1887, and passed eertain resolutions, as they were entitled to do, and among others that eomplained of by the plaintiff." Such being the rules and defenee, what aetually happened? On January 31 the secretary wrote to the plaintiff saying that he had much pleasure in informing him that he had been elected surgeon to the hospital. All went on happily and pleasantly until April. On April 6 the plaintiff was also appointed to the Margaret-street Infirmary. On April 30 he reeeived a letter saying there was a feeling against him consequent upon his conneetion with the Margaret-street Infirmary, where homœopathy was praetised. (It was perfectly optional there whether they treated the patients by homœopathy or not.) The same day he heard from the seeretary saying he would reeeive a notiee calling attention to that appointment. On May 5 a eommittee meeting was held and a resolution was proposed that no member of the staff of the hospital should profess or practise homœopathie doetrines, or be eonneeted with any homœopathie establishment, or any plaee where it (homœopathy) is recognised. This resolution was divided into two; both were earried, the plaintiff, who was present, voting for the first half, but against the seeond. He was then asked what he intended to do with regard to these resolutions; in reply he wrote a long letter, in whieh he declined to recognise the effeet of the resolutions, or to give way to what he considered tyrannical oppression and unmerited censure on himself. He also eommented on the assertion made by one of the members, that "Every homœopathic praetitioner was a conscious fraud, a liar, and an imposter." On May 26 a most extraordinary course was taken; there were seven present, and it was proposed that the entire business of the eommittee of management should be delegated to a committee of ten, leaving out the plaintiff. They aetually took on themselves to delegate all their power. It was, he thought, a well-settled rule of law that one person eould not delegate his duties to another. This delegated committee then earried a resolution suspending the plaintiff from his duties. Anything more eontrary to deeeney and rules of law could not be well eoneeived. The plaintiff was informed of this on the 27th, and on the 30th his suecessor was appointed. He held a strong opinion that the whole proceeding was wrong from beginning to end, and eontrary to the meaning of the rules. There was no neeessity for sueh instant aetion, and for them to take the law into their own hands was a flagrant and palpable abuse of their offiee. A more extraordinary or more improper proeeeding had never been done, so he would grant an injunetion. As to damages, the plaintiff said he would be eontent with merely nominal

damages. In his interests he thought he was wise not to ask for damages, but an order would be made restraining the defendants from interfering with the plaintiff in the performance of his duty as one of the surgical staff, and from suspending him from the duties of such office. The injunction would be granted with costs.

Mr. Pollock applied for a stay of execution, on the ground that the hospital would be shut up if the injunction were granted.

His Lordship refused the application.

THE APPEAL.*

The appeal was heard before the Master of the Rolls, Lord Justice Fry and Lord Justice Lopes, and was reported in *The Times* of the 16th January, as follows :—

This was an appeal by the defendants, who were members of the committee of management of the Queen's Jubilee Hospital, from the judgment of Mr. Justice Manisty at the trial granting an injunction restraining the defendants from interfering with the plaintiff in the performance of his duties as medical officer of the hospital by suspending him. The Queen's Jubilee Hospital was founded by Mr. Robert Fitzroy Benham, there being numerous subscribers of one guinea each, who were governors of the hospital. The hospital was opened in January last, and under the rules of the hospital the staff was to consist of a president, vice-president, patrons, patronesses, governors, trustees, treasurer, a committee of management, a secretary, an efficient medical and surgical staff, &c. Rule 7 provided that the government of the hospital should be conducted by a committee consisting of an equal number of the medical and surgical staff and lay members, all of whom must be governors of the hospital. Rule 8 provided that the committee of management should appoint as many physicians and surgeons as might be desirable for the efficient care of the patients ; the appointment should be for one year, and should be subject to confirmation by the governors. Rule 15 provided that an annual subscription of one guinea should constitute a governor. In January, 1887, the plaintiff was appointed one of the surgeons in the hospital, he being also a subscriber of one guinea, and therefore a governor. The plaintiff joined another hospital, the Margaret-street Infirmary, where homœopathy was allowed, and in some instances, practised, and, in consequence, at a meeting of the committee of the Queen's Jubilee Hospital on the 5th of May, two resolutions were passed—the first that no member of the staff of the hospital should profess or practise homœopathic doctrines ; the second that no member of the staff should be connected with a homœopathic establishment or with any institution in which homœopathy is either a recognised or an optional mode of treating the sick, or at which avowed or known homœopathists are office holders. The plaintiff, who was present, voted for the first resolution, but against the second. In accordance with these resolutions, the plaintiff was asked to resign his appointment as surgeon in the Jubilee Hospital, and on his refusing to do so, the committee on the 26th of May suspended him. The plaintiff

* We give the Report of the Appeal here for convenience sake ; but it will be understood that a large proportion of the letters were written before the date of its hearing.

thereupon brought this action, claiming an injunction and damages. Mr. Justice Manisty held that no damages had been proved, but granted the injunction set out above. The defendants appealed.

Sir Henry James, Q.C. (Mr. Edward Pollock with him), for the defendants, contended that, as no question of property arose, the Court could not interfere by way of injunction. This institution was a purely voluntary institution founded for charitable objects. The plaintiff as medical man received no salary. The plaintiff was a subscriber of one guinea to the hospital, and thereby became a governor. His position as governor had not been interfered with, but his voluntary position as medical officer had been interfered with. The effect of this injunction would be to compel the defendants to carry on the hospital. No Court of Equity would ever have granted an injunction in a case like this. He cited "Rigby v. Connell" (14 "Ch. D.," 482) and "Johnson v. Shrewsbury and Birmingham Railway Company" (3 "D. M. and G.," 914).

The plaintiff appeared in person, and contended that there was a question of property in this case, as his position of governor could not be separated from his position as medical man. His appointment as medical man was for a year, and there was a binding contract that he should continue as surgeon for a year. There were certain privileges attaching to the position of a medical man in a hospital which the defendants had interfered with. This institution possessed property, which he by his subscription had an interest in, his position in that respect being similar to the position of members of a club.

The Court allowed the appeal.

The Master of the Rolls said that the principal claim was for an Injunction, and the question was whether the plaintiff was entitled to that remedy. The Court had only to deal with the legal aspect of the case, and had to give no opinion as to the propriety, fairness, or sense of what was done by either side. The question was what rights, if any, the plaintiff had. The plaintiff said that he had the same rights as a member of a club. When a club was formed, it was by means of the subscriptions of members, which were expended in the purchase or hire of certain property, and each member had a right to use that property and had an interest in it. Upon that ground Courts of Equity had interfered in those cases. Looking at the rules of this hospital, it appeared to be plain that the plaintiff in his position as surgeon had no legal or equitable interest in any of the property purchased by the subscriptions. If the plaintiff had any interest in the property, upon which his Lordship gave no opinion, it was in his position as subscriber, in which position the plaintiff had not been interfered with. As medical officer, he would have a licence to use certain rooms and certain furniture in the hospital. He would have only a licence, and not an interest in the property. It followed, therefore, that, whether the relation between the plaintiff and the defendants was a contractual relationship or not, the relation was a strictly personal relation. Even, therefore, assuming that the defendants committed a wrongful act in putting an end to that personal relationship, the authorities were clear that the plaintiff was not entitled to an injunction. The plaintiff must bring his case within the principles upon which Courts of Equity will give such relief. Assuming, then,

that there was a contract here, Courts of Equity never dreamt of enforcing by injunction agreements that were strictly personal in their nature. Such was the decision of Sir George Jessell in "Rigby v. Connell," and the same doctrine had been laid down by Lord Justice Knight Bruce in the Court of Appeal in "Johnson v. the Shrewsbury and Birmingham Railway Company." No interest in any property having been interfered with, and the relation being a strictly personal one, the plaintiff could not get the relief claimed. There were other reasons why the plaintiff must fail, "Pickering v. the Bishop of Ely" (2 "Y. and C. Ch. Cas.," 249) showed that if one party could not have this remedy against the other, the latter could not have it against the former. It would be monstrous to say that the committee could force the plaintiff to continue on as a medical man and attend the patients in the hospital against his will. There was another reason, and that was that Courts of Equity would not give their own peculiar remedy where there was a perfect remedy at law. If there was a contract here, damages at common law would afford a perfect remedy. For these three reasons the plaintiff must fail. It remained, then, to consider his claim for damages. He gave no evidence of any, but if there was a contract and a breach of it, he was entitled to nominal damages. That, however, only affected the question of costs at the trial, and it was unnecessary to consider whether there was a binding contract or not, as the Court had come to the conclusion in the exercise of their discretion that there should be no costs in the Court below, the plaintiff to pay the costs of the appeal upon which he had been unsuccessful.

Lord Justice Fry concurred. Enormous inconvenience would be occasioned if Courts of Equity were to enforce the continuance of strictly personal relations when those relations had become irksome, and enforced them under penalty of imprisonment for contempt of Court. That would be too gross an interference with the liberty of the subject, and upon that ground Courts of Equity had refused to enforce them. The position of the plaintiff was twofold. He was a subscriber to the hospital, and so a governor, and that position was not interfered with. He was also a medical officer. It was not necessary to consider whether the appointment constituted a contract with him or not. But if it did, it created a purely personal relationship. There was sometimes a confusion between two classes of cases, one class where the parties contributed funds which were laid out on property which all enjoyed in common, such as clubs, and the other class where the contract created a purely personal relationship, such as master and servant. If in the latter class of case the party had suffered any injury, he had a right to damages only. The plaintiff's position as a medical officer being a purely personal one, Courts of Equity would not interfere.

Lord Justice Lopes gave judgment to the same effect.

[Lord Grimthorpe's first letter on the subject appeared in *The Times* of the 24th December, immediately after the trial of the action.]

LORD GRIMTHORPE (1) *

[*Dec.* 24.

The report of a trial of "Millican *v.* Sulivan and others" (the Committee of a certain "Jubilee Hospital") deserves more attention than it will probably receive from unprofessional readers—*i.e.*, neither lawyers nor doctors—unless its importance is pointed out. As the facts are all stated in the judgment, which anyone can read for himself, it is sufficient to say that an injunction was granted against the committee who were trying to turn out the plaintiff (of whom I may say I know nothing) from that hospital, of which he was a physician, elected under no restriction as to the nature of his practice, for the offence of being connected with the Margaret-street Infirmary, of which the governors had lately prevented their committee and the majority of its doctors from turning out a minority who had dared to treat their patients homœopathically.

It was my unexpected fate to be in the chair of several adjourned meetings of the governors of that same Margaret-street Infirmary, last winter, which had been called by the committee to assist the majority of themselves and the doctors in executing, or at least depriving, two or three heretics who had presumed to prescribe homœopathic medicines for their patients there. I had then no opinion in favour of either system beyond a natural leaning to small doses, which, at any rate, take nothing out of you, and can do no harm, provided only they will do the business. Whether they will or not I should soon find out for myself in any particular case if I tried them, and should act accordingly. What I learned then, I confess, turned my inclinations towards heterodoxy; not that that is or was the real question, but whether a certain set of doctors and poor patients were to be arbitrarily sacrificed to what has been well called "*odium plusquam theologicum* because *medicum*." After hearing both sides, the meeting decided just as Mr. Justice Manisty did, and I do not think his strong condemnation of the Jubilee Committee a bit too strong for either case. I called upon the ejectors to show cause why we should assist them, and a more miserable attempt at showing cause I never heard. They declared that they were not come there to discuss the merits of homœopathy, or even to argue whether it was or was not successful, but repeated ever so many times, as quite sufficient for ignoramuses like us, that it was "not recognised by the profession." "Very well," I said, "then show us where and how it has been condemned by any body or council authorised to represent the profession;" and I reminded them that the 23rd and 52nd Sections of the Medical Act, 1858, had done all that Parliament had then thought sufficient to prevent any exclusion from practice or inflicting any stigma on any such ground, and enacted that if any licensing body

---

* One has no opportunity of correcting proofs of letters to a newspaper, except in very special cases. I never see a proof without wanting to correct it, sometimes for serious mistakes of either writing or printing. One such occurred in one of these letters, which, though corrected the next day in a paragraph, furnished a topic for several letters from the least scrupulous of the writers on the other side—"J. C. B." It could not now silently be withdrawn without mutilating several of my letters in reply to him, and therefore I have to point it out in its proper place. I have also made a few alterations and additions in consequence of information which kept coming in during the course of the discussion, or to make a few points clearer.—G.

did so, it should be deprived of its powers by the Privy Council. There could hardly be a stronger legislative condemnation of medical intolerance. They were utterly unable to produce any such authoritative, or even unauthoritative, condemnation, and one of the minority read something which sounded very like a repudiation by one of the authorised bodies of any such attempt; but I have no means of quoting it accurately now. It would have been flatly in the face of the plain intention of the Act if there had been.

In order to give the committee every possible opportunity of justifying their action, I asked if there had been any complaints from, or on behalf of, patients who had been treated by the accused doctors, as the others had issued a manifesto which ordinary readers would, and did, understand as a charge of incompetence, though I did not think it grammatically was so. That intention also was disclaimed; and so the whole charge sank into nothing but that the majority of them, and I daresay of the whole profession, disbelieve in the views, or theory, or treatment, of this not inconsiderable minority, just as they did once in the circulation of the blood, in vaccination, and in fresh air in sick rooms; and as the fashionable treatment of one decade of medical practice, and even their fashionable wines, are condemned in the next by the leaders, and forthwith by the juniors, of the profession. Most of us of sufficient age have known certain surgical treatments denounced as "quackery," and the "quacks" pronounced impostors, and afterwards recognised as having committed no sin but being ahead of their leaders, which, no doubt, is presumption, always duly snubbed at medical consultations. As these people evidently mean to defeat the Medical Act by the roundabout process of closing every hospital against those whom they are prohibited from excluding from private practice, the time is come when the governors or subscribers must decide for either liberty or tyranny. If the tyrants like to abdicate when they are beaten, as they did at Margaret-street, the gaps will speedily be filled. Nobody ever yet conquered by abdicating, if the other side knew their business and were resolute. Hospitals do not belong to the doctors; they exist for the double purpose of relieving the poor and teaching doctors by the experience they gain there for the benefit of themselves and of all classes. If committees are so cowardly as to allow whatever happens to be the medical majority of the day to prescribe not only medicines but what *medici* are to be allowed to practice there, and to exclude some who have gained all sorts of distinction in their schools, it is time for one of two things—either to dethrone such committees, or, at least, to treat them as Mr. Justice Manisty and the Margaret-street governors did; or else to make the next Medical Act still stronger in favour of liberty and against tyranny and prejudice. We had to pass an "Act for the submission of the" doctors of theology three and a half centuries ago, which they have been constantly trying to repeal by all sorts of tricks, and are at work upon again now, I know. We must have another for the submission of doctors of medicine, who are now frightening timid and ignorant committees, as the others did timid and ignorant kings, from the time of Stephen to that of Henry VIII., into conceding them an entirely unconstitutional dominion.

GRIMTHORPE.

Lord Grimthorpe has pointed out that the 23rd section of the Medical Act of 1858 expressly provides against the refusal of its licence by any medical licensing body to a candidate on the ground of his adoption of any special tenet or theory of medicine. It may possibly help us to appreciate the present position of things if we glance briefly at the origin and growth of the *odium medicum* in this particular.

When first the doctrine (or, rather, the therapeutic rule)—"let likes be treated by likes," was enunciated by Hahnemann, it was received by the medical profession at large with derision, and scouted as an axiomatic absurdity. Such being the state of things, one section in the medical world, claiming as its only principle in therapeutics a rule which the other section regarded as both impossible and absurd under any circumstances, it was obvious that a consultation between members of the rival schools must lead in the very nature of things to one of two results—either a hopeless disagreement or the entire sacrifice by one or other practitioner of his principles. In the former case it would be unfair to saddle the patient with the useless and unnecessary expense of a consultation that must inevitably be barren; in the latter the consultation would be equally a useless and unnecessary expense, and, in addition, a sham and a fraud. Moreover, at a time when a homœopath was practically pledged to infinitesimal fractions of a drug for a dose, while the "regular" profession did not believe that any action at all was to be produced by small doses, there was another element which landed the proposer of a mixed consultation in the same dilemma as in the case of the doctrine above referred to.

But now all that is changed. We of the "regular" profession admit that there are individual cases where a drug, which in the healthy body in large doses will produce certain symptoms, will, in small doses, cure similar symptoms arising from the disease. For instance, we have it on Dr. Ringer's authority (and very many of us can verify it from our own personal experience) that drop doses of ipecacuanha wine (in large doses a popular emetic with which even most mothers of families are acquainted) will check many forms of obstinate vomiting. But grant one instance, and the contention that the law is an axiomatic absurdity falls to the ground. The difference becomes no longer one of first principles, no longer one of kind, but one of degree; consequently there is no predetermined impossibility of an honest agreement in consultation as to the drug indicated in a given case. The same line of argument holds good in the question of dosage; for while many homœopaths discard dilutions and infinitesimal doses, we have on our side learnt that, although always using the stronger preparations of drugs, there are many cases in which single drops or grains, or even fractions of a drop or grain, will have a distinct therapeutic effect.

There is, therefore, no longer on this score either, that predetermined impossibility of an honest agreement in mixed consultations which was the justification, or, at least, the excuse, for our action in the past. But the enmity and rancour aroused by an exchange of the lie direct remain—on their side since they see us admitting bit by bit,

but without apology or explanation, some of the points we have hitherto denied; on ours, as is often the case, from the assailant to the assailed. There is, of course, still a difference, and in many respects a marked one, between the two schools, but it is a difference of degree more than of kind, and consequently one not hopeless of a final harmonious adjustment if approached in a conciliatory manner on both sides. Overtures have frequently been made from the other side, but, having proclaimed aloud our *non possumus*, we will not retract. And so we find some other reason for not withdrawing our excommunication. The homœopaths have broken the unity of the profession. They have become sectarians, "trading on a name," with an organisation, societies, hospitals distinct from and opposed to those of the "regular" profession. This separate existence is, as Dr. Austin Flint some time back expressed it, an assumption that they "represent an essentially distinct system of practice, taking an attitude of antagonism to the regular profession, seeking popular favour on the ground that they belong to a 'new school,' based on truth and productive of good, whereas the regular profession belong to the 'old school,' based on error and productive of harm." (*New York Medical Journal*, April 7, 1883.) It is very convenient and soothing to forget that by the acrimonious way in which we originally gave them the lie direct we forced them into that position of antagonism, and that the very ostracism to which they have been for years, and are still, though fortunately to a less extent, subjected, scarcely tends to abolish that antagonism.

In the same journal, Dr. Austin Flint continues: "If homœopathic practitioners abandon the organisation and the name, provided they have received a regular medical education" (which in this country they are by law compelled to do, and that in the schools of their opponents, moreover), "there need be no restrictions on consultations other than those belonging to other portions of the code" (the American Code of Medical Ethics) "whatever therapeutical doctrines they may hold." This view of the case, moreover, has been enunciated and maintained in this country by our own medical journals. and by several of our medical "men of light and leading." All that the homœopaths have to do, therefore, to be once more restored to professional fellowship, we proclaim, is to abandon their organisation and their name, disband their societies, cease to circulate special directories, and dismantle their hospitals, and thus discard their attitude of antagonism to the "regular" profession. Indeed!

I cannot comment on the case of Millican *v.* Sulivan and others, the Committee of the Queen's Jubilee Hospital, as it is still *sub judice*, but I may fairly suggest to anyone interested in this subject to read the account of the action.

KENNETH MILLICAN.

58, *Welbeck-street, Cavendish-square, W.*

Sir,—As founder and the one who appointed Mr. Millican to the throat department of "The Queen's Jubilee Hospital," will you permit me most emphatically to contradict Lord Grimthorpe's statements with reference to this institution?

The facts of the case have not yet been heard, and I am not permitted to enter into any controversy on this subject until after the hearing of the appeal which is now pending. THE FOUNDER.

[The appeal was heard on January 14, when the former judgment was reversed on a point of law.—See report of the hearing on page 9.]

LORD GRIMTHORPE (2).

[*Dec.* 29.

If the angry "Founder" had been content to say that he and his managers were going to appeal against the judgment which pronounced their conduct "a flagrant and palpable abuse of their office, and as contrary to decency and rules of law as can be conceived," and granted an injunction against it, I should have been silent till the appeal is decided or abandoned. But as he breaks his own rule against controversy by "giving an emphatic contradiction to my statements," which is a tolerably strong piece of controversy, I beg to ask him which of them he means to call false; for of course he means my statements, not Mr. Justice Manisty's—at least that is not yet a customary or very safe mode of speaking of judgments. I daresay he and his managers dislike both the language and the substance of the judgment. But if "the facts were not heard" that must be their own fault. Their counsel are not likely to have dropped any that would do them good.

But I have something to say which has nothing to do with their appeal. I believe I was wrong in saying that if such people as these and the late Margaret-street committee and doctors set to work to defeat the Medical Toleration Act by shutting up hospitals against competent medical practitioners we must have a new one. Conspiracy at common law—luckily not spoilt by statute-mongering—has a very wide net indeed, and the law books say it is difficult to fix its limits. So much as this is certain however, because it has been decided, or laid down in books of unquestioned authority: it is conspiracy at common law, punishable by fine and imprisonment, practically at the discretion of the Court, to combine to injure a man in his business or his reputation; to compel him to carry on his business in any particular way, or not to do so; to employ or not to employ particular people; or to do any act injurious to third persons or the public or any part of them; or to make false charges not only of legal offences but of anything discreditable—assuming in every case that justification cannot be proved.

If a quarter of the things I have been told or read are true, of the persecution of those whom the regulars of the medical inquisition call "quacks"—but never can define what they mean—or "bone-setters" who have mended the limbs of thousands of people after the best regulars had failed, the persecutors sometimes even boycotting those who worked for such heretics, I believe they would fare very badly if they were indicted for conspiracy before a strong judge like Lord Campbell, who boldly overruled some hair-splitting technicality which a previous judge had allowed in a similar case. And now I see they have advanced a step further; for Mr. Millican appears not even to be guilty of the crime of homœopathy himself, but only of keeping company with unbelievers in the infallibility of the allopathic

blue-pill and large-dose men, at the Margaret-street Infirmary. Like all tyranny, that must go further; any doctor who meets Mr. Millican in consultation must be boycotted too on logical principles. But let the homœopathists get up their case well and get a summons before a magistrate, which is necessary for an indictment for conspiracy, and boldly prosecute their persecutors. GRIMTHORPE.

DR. THUDICHUM.

[*Dec.* 29.

The attempts of Lord Grimthorpe and Mr. Kenneth Millican to make the public take an interest or a side in a case of questioned professional ethics arising out of disputed doctrine will probably remain without any useful result. For my own part, and of those who co-operate with me, I deprecate the discussion, first because, so far as disputed doctrines are in question, the medical profession have long since definitely spoken, and not in the sense of those whom Mr. Millican describes as "we of the 'regular'" profession; secondly, because an appeal against the late judgment is pending, being the fourth act of the forensic drama by which Mr. Millican has manifested his strong desire to be one of the surgeons of the Queen's Jubilee Hospital; and thirdly, because both Lord Grimthorpe and Mr. Millican put forth statements which do not bear a moment's inquiry. Against some of the former the founder of the Queen's Jubilee Hospital has already entered his protests; some of the latter can be judged of by the following.

Mr. Millican says that "we of the 'regular' profession" admit the allegation first made by homœopathists "that drop doses of ipecacuanha wine will check many forms of obstinate vomiting."

The history of this allegation has been recorded, and the allegation itself refuted by the late Sir James Simpson, the discoverer of the use of chloroform, in his work "Homœopathy, its Tenets and Tendencies" (3rd edit., p. 274, note). I enclose the quotation in extract; it is an instructive bit of history which, having the medical question only in the back ground, can be appreciated by all your readers.

J. L. W. THUDICHUM, M.D.
Fellow of the Royal College of Physicians, London, Chairman of the Board of the Queen's Jubilee Hospital, Gloucester Terrace.

"In the *British Journal of Homœopathy* for January, 1882, p. 159, is given a quotation from a pamphlet of a certain Dr. Stewart, accusing Dr. (afterwards Sir James) Simpson, the celebrated Edinburgh physician, discoverer of the anaesthetic action of chloroform, of having practised homœopathy in one instance; and the latter noticed it in his work 'Homœopathy, its Tenets and Tendencies' (3rd edit., p. 274, note), not, as he said, to defend himself against the allegation, but to give it as a specimen of the unscrupulous assertions regarding homœopathic cures and homœopathic practice. The quotation in the journal named is as follows:—'Towards the close of last winter's session, when Dr. Arneth, of Vienna, was in Edinburgh, he was in the habit of attending Dr. Simpson's lectures. On one occasion, in Dr. Arneth's presence, I recollect that Dr. Simpson told his class of a case of vomiting in pregnancy then under his care. He had launched forth

in succession all his allopathic appliances—opium, naphtha, prussic acid, &c.—(I forget if chloroform was also used),—but without relieving the patient. He had either taken Dr. Arneth to see it, or had told him about it, and the latter recommended him to try either a quarter or half a grain of ipecacuanha, I do not recollect which. He acted upon this suggestion and with decided good results.' The case here referred to was one under the care of Dr. Myrtle. At the strong recommendation of Dr. Arneth, who was with Dr. Simpson at the consultation (and whom he did not know at that time to be a practitioner of homœopathy), the patient got half a grain of ipecacuanha, but instead of producing 'decided good results,' as Dr. Stewart alleged, it made the patient very decidedly worse, and had to be speedily withdrawn.

"But (continues Simpson) mark the further progress of a homœopathic case. In the *British Journal of Homœopathy* for January, 1852, it is averred simply, though erroneously, that the use of the alleged homœopathic remedy, ipecacuanha, was followed 'with decided good results,' nothing more. In the same journal, however, for July, 1852, p. 468, the same case is again referred to: and now it is deliberately and quietly stated, as an actual homœopathic fact, that this was an instance of vomiting in pregnancy 'which he (Dr. Simpson) could not cure, until he gave the patient very small doses of ipecacuanha, whereupon the vomiting cased.' Simpson has stated (*i.e.*, p. 274), in the name of Dr. Myrtle and himself, that upon the use of the ipecacuanha the vomiting was not 'cured;' and that there were even 'no good results' from it; but, on the contrary, the very reverse and opposite."

MAJOR W. V. MORGAN. [*Dec.* 29.

The letters of Lord Grimthorpe and Mr. Millican will enlighten that portion of the public unacquainted with the position taken up by the old school of medicine.

Over and over again the new school, called by their antagonists the "Homœopathic," have offered to drop the name and discontinue their hospitals and journals provided fair play be conceded on the other side. Many years ago an offer of upwards of £20,000 was made by one bearing the honoured name of Gurney to any hospital which would devote a large ward to the treatment (under the inspection of its authority) of patients on the new principle. Again, when an appeal was made a few years ago by the Duke of Westminster for funds to enable St. George's Hospital to utilise its wards, the writer offered to subscribe £1,000 a year for five years on the simple condition that it should be expended in testing the system in one of the wards of that hospital. But no; rather would they keep the wards closed and let the poor patients die than assist at the dissemination of such heresy. Moreover, the discussion of the system, and even the advertisements of books bearing on the subjects, are rigorously excluded from all the medical periodicals. What then are those who, like myself, have a life-long experience of the system and an ardent belief in its efficacy to do? We have established a hospital with ninety beds in London, and support two periodicals, but would gladly drop these if only guaranteed fair play and a cessation of boycotting.

WILLIAM VAUGHAN MORGAN, Chairman of
5, *Boltons, S.W.* the London Homœopathic Hospital.

LORD GRIMTHORPE (3).

[*Dec.* 31.

I need hardly say I was prepared for the abuse of such papers as *The Medical Journal*, and such writers as Dr. Thudichum. I dare say other phlebotomists will have a cut at me too. But they are not Butlers or Whatelys, and therefore had better not meddle with the double-edged tools of analogy. They do not know that no less a person than Lord Brougham asserted his right to do the very thing that *The Medical Journal* says no barrister dare do, and nobody thought of meddling with him; and that Sir Fitzroy Kelly, as I have heard him tell at Lincoln's Inn, took his retainer in one of his greatest causes from his client on a steamboat going across the Channel, no solicitor being on board, and thereby anticipated the other side, who went to his clerk just too late. It is true that counsel seldom do so, because there are inconveniences in it. But it is quite a settled practice that they may take instructions direct from a prisoner in criminal cases. In Parliamentary work we often had to talk to clients of all kinds without waiting to get a solicitor to look on. And we once passed a unanimous resolution, in consequence of some assertion outside, that if counsel specially agreed to take less than the universally presumed daily "refresher," we had no right to interfere with it. I believe I moved it, though I was not then the leader of that Bar.

The real analogy that would suit this medical trade union would be if the Bar were idiotic enough to try and boycott some considerable minority of them who made speeches or examined witnesses in some way that the majority either disapprove of or are unable to imitate successfully. If the minority generally failed, that would soon settle itself; and so it would if homœopathists generally failed. But instead of that they increase, which proves that they do not fail, without anything more. I see from a pamphlet sent to me, containing some still worse specimens of unionistic tyranny, extending even to surgeons who give no drugs, large or small (of which indeed Mr. Millican's is a case), that there are above 10,000 homœopathic doctors in [England] the world.* There are two homœopathic shops, I know, within a short distance from my house, and I dare say more; and no large town is without some. And as for the private boxes of pillules kept for family use, nobody can tell the number within a great many thousands. It is only that kind of logic which I have long observed to come with a medical education that could dream of comparing this widely-spread heresy to isolated cases of disreputable or incompetent practice in that or any other profession.

In any other branch of physics (and why not in physic ?) the refusal to let an experiment be tried, which cannot hurt and is not objected to by those whom it concerns, would be taken as a decisive proof that the refuser fears or knows that it will refute his theory. What rational excuse can these unionists make for refusing the two munificent offers to found and maintain experimental homœopathic wards in two hospitals which were described yesterday ? Is there a man in England fool enough to believe that it was from a benevolent regard for hypothetical patients ? If I were a subscriber to St. George's Hospital, which did so very lately, I would withdraw to-morrow, to escape next

* So corrected in a paragraph the next day.

year's subscription. A very able and learned friend of mine used to complain to me of the medical tyranny there in other matters too, and one of the Homœopathic League Tracts, which have been sent to me, is full of gross instances of resolute determination of the doctors to put down homœopathy in every hospital that is not specially devoted to it, that is, in all the general and old and great ones. Any experiments may be tried on the patients except that, which they plainly are afraid would succeed and be fatal to their own reputation. There are some very queer revelations about hospital practice in a new book called "St. Bernard's," which could be written by no one who has not been initiated. What right has Dr. A. to interfere with the way Dr. B. cures his patients? If he generally fails people will find it out fast enough. In no kind of physiology is any proof worth a farthing except the results of experience. I have read that such experiments have been tried, under State protection against medical tyranny, in some foreign hospitals, and that the results were exactly what these people manifestly fear they would be.

Dr. Thudichum's confused ipecacuanha story, 36 years old, has been sufficiently dealt with by Mr. Millican, except as to one awkward fact which the doctor is a little over-zealous in parading. The infinitesimal dose in that case not only did not cure the sickness but made it worse. Did it? I thought the essence of the anti-small-dose case was that such doses do nothing, and that the only way they can seem to do good is by saving the patients from being drenched with large ones, which means that the large ones in all those cases would have been worse than nothing. That was the line of argument in Sir Benjamin Brodie's article in the *Quarterly* long ago, which I have read again and find as illogical as I remember thinking it at the time, and now also more refuted by experience.

These writers very naturally "deprecate discussion," except their own. "The medical profession have long since definitely spoken." *Roma locuta, causa finita est.* If it is worth asking again, as I did in vain at Margaret-street—When, where, how, and by what authority? They have given me, and therefore Mr. Justice Manisty, "an emphatic contradiction," and that ought to be enough for us and the public. They affect to treat my "boisterous rhetoric" with contempt, and say "the public will take no interest in it." Then they need not be in such a fury at it. I have opposed and defeated a good deal of trade unionism in my time—of clockmakers, architects, and even of ladies, who all tried to beat me by abdication or secession; and I have lived to see workmen's unions, against which I wrote ten years ago, confess their failure to do a great deal that they expected. I am not frightened of being washed away by black doses from a set of medical conspirators.

GRIMTHORPE.

DR. T. F. PEARSE.

[*Dec.* 31.

Certain interested individuals have been attempting to obtain capital in favour of homœopathy from the trial of "Millican v. Sulivan and others" still under jurisdiction. Unfortunately for them the matter at issue is quite outside the incompetency of homœopathy or its merits.

It resolves itself into whether an institution favouring homœopathy is or is not likely to get the support and good will of the medical profession. Scientific medicine has over and over again rejected the principles and practice of homœopathists, and therefore the Committee of the Queen's Jubilee Hospital, desiring that support, did not consider it politic to retain a member in Mr. Millican, who had practically thrown in his lot with the homœopathists.

Happily for charitable institutions rejected members do not always behave so enthusiastically in opposition as in this particular case, and one cannot help drawing an inference that sympathy with the homœopaths and prospects of benefit must have actuated the opponents of the Jubilee Hospital.

T. FREDERICK PEARSE, M.D., M.R.C.P.,
Physician to the Skin Department, Queen's Jubilee Hospital.

R. B. C. (1).

[*Jan. 4.*

The promoters and managers of the so-called "Jubilee Hospital" have much reason to be grateful to Lord Grimthorpe for his exceedingly comic outburst of indignation against "doctors," and for the "bold advertisement" this indignation has given to the ricketty bantling which the aforesaid promoters and managers are striving to guide through many surrounding perils. With the precise issues raised in the law suit which excited Lord Grimthorpe's wrath I have nothing to do; and I have no wish to occupy space in the discussion of such a tempest in a tea-spoon; but I would like to protest against the suggestion of his lordship, surprising from one who is himself so prompt to exercise an independent judgment, that the views expressed by Mr. Justice Manisty are conclusive on the merits of the case. I have not the honour of knowing Mr. Justice Manisty, but I entertain no dobt that he is a sound and learned lawyer, and unless or until his decision should be reversed in the appeal said to be pending I am content to accept that decision as showing that the plaintiff before him had sustained a legal wrong. To this amount of deference a judge is clearly entitled from non-legal persons; but on the point whether the defendants had committed any other wrong than a technical one I should feel entitled, if I were sufficiently interested in the question, to form my own opinion after inquiry into the precise circumstances of the case.

Passing away from this really trivial matter, I would ask your permission to explain, and to endeavour to justify, the attitude of the medical profession towards homœopathic practitioners. Technically these persons are members of the profession, practically they are not; and, without in the least questioning their legal *status*, it will be convenient to use the word homœopaths, to designate medical men who profess to follow homœopathy, and the word "doctors" to designate medical men who would entirely repudiate such an imputation.

Homœopathy, properly speaking, is a body of doctrine which was invented by Hahnemann, and which, after some preliminary publication

in 1796 and in 1805, was finally put forth in 1810, in a work entitled "Organon der rationellen Heilkunde," as comprising the whole philosophy of medicine. It was not the fruit of study or research, but was evolved by the inventor out of his moral (or immoral) consciousness, and it rested mainly upon the following assertions:—First, that all disease depended upon a sort of evil principle called "psora," or, in plain English, "itch," which pervaded the system of the sufferer and produced symptoms, so that disease was really latent or suppressed itch. (In those days it was not known that real itch is caused by a parasitic insect which burrows in the skin.) Secondly, that all the symptoms of disease could be relieved or cured by the administration of medicines which would produce similar symptoms in a healthy person. Thirdly, that these medicines, in order to produce their curative effects, must be given, not in small, but in infinitesimal doses—that is to say, in doses ranging from the millionth to the decillionth of a grain. Fourthly, that the potency of the medicine increased in proportion to the diminution of the dose, and that it also increased in proportion to the number of rubs in a mortar or shakes in a bottle which were employed in making the mixture from which the diminished dose was to be taken. In diluting solids powdered sugar of milk was employed, in diluting liquids water; and a grain and a drop may be taken to be equivalent terms. For example, a spirituous solution of opium—*i.e.*, laudanum, was prepared for administration by adding one drop of it to one hundred drops of water, and by giving the containing bottle three shakes of a specified character, each produced by one movement from the elbow. Hahnemann said that he at first permitted ten shakes, but that the power of the medicine was thereby so dangerously increased that he found it necessary to limit the number to three. The mixture thus made, however, even if much shaken, would be of very restricted utility, and it had to be further diluted in order to develop its powers. One drop of the first dilution, or, as it was called, "attenuation," was mixed with another hundred drops of water, and this second mixture received its three solemn shakes. It was then in a state to yield one drop, presumably containing one ten-thousandth of the original medication, to be mixed in the same way with another hundred drops of water, so that each drop of the result after three shakes might contain one-millionth; but in the case of opium the mixture was still not strong enough to be employed against serious disease, and the attenuations were continued until the thirtieth was arrived at, in which each drop of the result was supposed to contain one-decillionth of the original laudanum. As illustrations of ordinary doses, I may mention that "charcoal" was directed to be given in one or two decillionths of a grain; "chamomile," in two quadrillionths; "nutmeg," in two-millionths; "tartar emetic," in two-billionths; "opium," in two-decillionths of a drop of laudanum; "arsenious acid," in one or two decillionths; and "ipecacuanha," in two or three millionths.

The late Mr. Heckstall Smith once published a striking calculation to show what the decillionth of a grain or drop would mean. He took the orbit of the planet Uranus, and found that if he had a cubic measure, each side of which was equal to the square of the diameter of this orbit, it would be necessary to have this measure filled eleven

times and a half in order to obtain sufficient water to dilute a drop of laudanum or other liquid in such a degree that each drop of the mixture would contain a decillionth of the drug. In the same way, the bulk of sugar of milk required in order so to dilute one grain of "nutmeg" that each grain of the mixture should contain a decillionth of a grain of the spice, would be greater than the whole bulk of the universe of which the earth forms part. Apart from this *reductio ad absurdum*, it is tolerably manifest that the three shakes would not suffice to produce a uniform mixture of two fluids of different specific gravity, and that the drop of the first attenuation could scarcely fail to contain either more or less than one-hundredth part of the original medicine.

Without dwelling upon the question of infinitesimal doses, it is, perhaps, more important to point out that the fundamental basis of homœopathy, the doctrine that symptoms are to be cured by medicines that would produce them, is directly at variance with the principle which is the guiding light of modern medicine. That principle is that it is the business of the physician to go behind symptoms to the morbid changes which are underlying them. There is a science called pathology, which deals with the exact nature of those departures from natural structure or from natural function which constitute disease, and with the means by which such departures may be rectified or brought under control. To the advancement of this science every department of physical research has been made contributory; and its progress has rescued mankind from the dominion of many evils, and promises to rescue them from more. If a patient has a cough, the business of the doctor is to find out why he has a cough, and then to apply the appropriate means for his relief. His cough may be due to an affection of the throat, to an affection of the lung, to stomach derangement; to irritation of some distant nerve; to the pressure of some internal tumour, to tubercle, or to cancer; and the doctor has to find out which of these causes, or of others, is in operation, and then to take his measures accordingly. Of course, he may arrive at an erroneous conclusion, just as, I suppose, a lawyer may sometimes form an incorrect opinion. But the homœopath ignores the whole question of pathology:—

> "A primrose by the river's brim,
> A yellow primrose is to him,
> And it is nothing more."

His patient coughs, and all he has to do is to look in one of his books to find out what medicine will produce cough in a healthy person, and then to administer this medicine, if he be very bold, in a dose so energetic as one or two decillionths of a grain.

Perhaps the drollest part of the whole homœopathic hypothesis is the assumption that there is any real resemblance between the actual symptom presented by disease, and what I may call the sham symptom which is alleged to be produced by the curative agent. Belladonna was adopted as the cure for scarlet fever because it was said to produce a red rash upon the skin. Belladonna was also recommended by Hahnemann as a cure for hydrophobia, because it produces a dryness of the mouth which leads the patient to make efforts to relieve himself from the annoyance of viscid saliva. But there is no real resemblance

between the belladonna rash and the rash of scarlet fever, nor between the viscid saliva produced by belladonna and the condition produced by hydrophobia. Nor, after the lapse of eighty years, has there been any instance of a cure of either disease by the reputed remedy.

There is, of course, no limit to the erroneous opinions which people may entertain about subjects on which they are profoundly ignorant; but the absurdities of homœopathy were from the first so patent to all who had received the benefit of a medical education that these persons might well have been excused if they had entirely declined to "investigate" such nonsense. They might have rested upon the well-known dictum of Faraday, to the effect "that a man who makes assertions, or draws conclusions, regarding any given case, ought to be competent to investigate it. He has no right to throw the onus on others, declaring it their duty to prove him right or wrong. His duty is to demonstrate the truth of what he asserts, or to cease from asserting. The men he calls upon to consider and judge have enough to do with themselves in the examination, correction, or verification of their own views. As life is limited, I am not disposed to occupy the time it is made of in the investigation of matters which, in what is known to me of them, offer no reasonable prospect of any useful progress, or of anything but negative results."

The medical profession, I say, might justly have rested upon the principle here laid down, but as a matter of fact the truth of the homœopathic doctrines was early brought to the test of experiment. An application was made to the French Government for permission to establish a homœopathic dispensary in Paris, and the question was referred by the Minister of Public Instruction to a committee of the Académie de Médicine, presided over by the illustrious Andral. On March 17, 1835, the committee reported to the Académie. M. Andral said he was decidedly opposed to the project of allowing the homœopaths a dispensary; humanity should not be trifled with by the experiments of these people. He had given their system a fair trial; he had treated about 130 or 140 patients homœopathically, in presence of the Hahnemannians themselves. M. Guibourt had prepared the medicines, and every requisite care and precaution were duly observed, yet in not one instance was he successful. He had tried various experiments on his own person, and several professional friends had followed his example, in order to ascertain the actual effects of homœopathic doses; but the results were not as Hahnemann and his disciples described them. He (M. Andral) had taken quinine in the prescribed globules, but had contracted no intermittent fever; he had taken aconite, but without being affected with symptoms of plethora; sulphur he took, to try if he could catch the itch, but he caught nothing; neither upon swallowing certain globules of arnica did he feel pains as if he had suffered contusion; and so with various other substances which he and his friends took in obedience to the Hahnemannian precepts. With respect to the attempt to cure disease by this method, he said that in every instance the symptoms went on from bad to worse.

In the discussion which followed, M. Piorry dwelt upon the absurdity of overlooking the seat of the disorder. M. Bally said that he had given permission to MM. Currie and Simon to treat patients homœo-

pathically in his wards ; they had done so for four or five months ; a register was kept, and the result was that not one of a large number of patients was cured by the Hahnemannian method. After similar speeches from others, the Académie unanimously advised the Minister to withhold his sanction from the proposed dispensary.

There was thus, more than fifty years ago, an abundance of the experiment which Lord Grimthorpe desires ; and doctors had no difficulty in making up their minds with regard to the true character of the homœopathic system. To put the matter quite plainly, its absurdities, to any who have received a medical education, are so manifest that we do not believe such persons, if of ordinary mental capacity, can honestly believe them ; and we have abundant evidence that many who profess to be homœopaths do not carry their profession into practice, but prescribe ordinary remedies in ordinary doses, and only call themselves homœopaths in order to fleece some of the more gullible sections of the public. To the genuine homœopaths, if such there be, we object because we distrust their mental capacity ; and to those who are not genuine we object on grounds of ordinary morality.

I cannot refrain from pointing out that there is one almost crucial test by which the value of the system may be judged. For many years the members of the medical profession have been in the forefront among the cultivators of pure science, among those whose aim in life has been the establishment of truth, and they have enriched mankind with notable discoveries. The undulatory theory of light was the discovery of Thomas Young. In every department of physical research, in chemistry, in electricity, in natural history, they have been conspicuous. Both the Darwins were doctors, Dalton was a doctor, Huxley is a doctor, Hooker is a doctor. The fellowship of the Royal Society, the red ribbon of science, has been bestowed upon doctors in larger proportion than upon any other class of the community. In all this mental activity the homœopaths have been conspicuous only by their absence. There may be exceptions, although I am not aware of any ; but, save as an exception, no homœopathic practitioner, from the time of Hahnemann until now, has ever contributed a single fact to science, or left mankind one whit wiser for his having lived. The whole tribe has been smitten with that utter intellectual barrenness which, as a Nemesis, attends upon " whosoever loveth and maketh a lie."

We shall probably be told that patients recover under homœopathic treatment. The extent to which this may be true is greatly obscured by the fact already referred to, that many professing homœopaths do not practice homœpathy ; but it is no doubt true in some measure. The human body is possessed of great recuperative powers, and makes a gallant fight against disease. The most that the doctor can do, in a general way, is to assist nature in throwing off the burden, and she may often throw it off without being assisted at all. On the other hand, we see numerous instances in which a week of rational treatment would lead to recovery, and in which, if that treatment be withheld, illness may be protracted for months, and may even lead on to a fatal issue.

For myself, I beg leave absolutely to disclaim the imputation of *odium medicum*. I am as indifferent to homœopaths as if they were

inhabitants of another planet. I do not object to their being consulted by those who believe in them, nor to their hospitals or dispensaries for any who please to resort to these institutions. I should decline to consult with them, or to receive them as hospital colleagues, because I have no common ground of science on which I could meet them, and therefore I could not, as hospital colleagues sometimes must do, transfer my patients to their care. Lord Grimthorpe sneers at the suggestion that the refusal of the staff of St. George's Hospital to consort with homœopaths was produced by "benevolent regard for hypothetical patients;" but I should be perfectly content to be judged by my own hospital patients on the question of the genuineness of my solicitude for their welfare. What surprises me most in his Lordship's account of homœopaths is to hear of their abundance. I have been thirty-six years a qualified practitioner, the last twenty of which have been spent in London, and I am on the staff of two great hospitals, one general and one special. In the course of my life I have heard the names of three homœopaths, and I have seen two of them. They must be like the "shy animals" on which the late Mr. Jefferies wrote an interesting paper, saying that, although the majority of people did not see one of these animals twice in a lifetime, they were really more numerous in England and Wales than men, women, and children. As for the domestic practice of homœopathy, it will perhaps surprise Lord Grimthorpe to be told that, according to current report in the drug trade, the pilules sold to old ladies do not contain a particle of the various medicaments after which they are respectively named. They are made in bulk of pure sugar, and are distributed, when finished, into bottles bearing different labels.

I must make one exception to my profession of indifference. Lord Grimthorpe has, I think, entered upon this discussion without any accurate information concerning the facts, and he has been ludicrously unjust to the numerous members of a profession which certainly contains a fair proportion of highly estimable men. But "we are all proud of him." We admire his fearless, if sometimes random, assertions, and we relish his trenchant style. If ever he should have a serious illness and should commit himself to the care of a homœopath, I trust that the individual thus honoured may be one of those who employ globules for the sake of appearances, but who secretly practise rational medicine whenever they are confronted with urgent disease.

Having no desire for an advertisement, I will not sign this letter with my name; but my initials will hardly conceal my identity from members of my own profession.

R. B. C.

J. C. B. (1).

[*Jan.* 4.

It is clear that Lord Grimthorpe is not, any more than Dr. Thudichum or *The British Medical Journal*, a Butler or a Whately, and, therefore, had better not meddle with the double-edged tools of analogy. "The real analogy" which he draws between a supposed course of proceeding on the part of the Bar and the action of orthodox medical men in declining to consult with homœopathic practitioners is not a real analogy at all, but patently false and flimsy.

Orthodox medical men refuse to meet homœopathists professionally, I take it, not because these adopt unusual methods in investigating cases or assume peculiar arts of manner and expression in conveying their conclusions and recommendations—conduct which would be analogous to that of barristers who examined witnesses or made speeches in a way that the majority of their profession either disapproved or were unable to imitate ; but because they hold views which render conjoint deliberation impossible, or profess principles which they do not honestly believe. Would Lord Grimthorpe, when Sir Edmund Beckett, have held a brief with a learned brother who had persuaded himself that water will run uphill, or that hearsay is more trustworthy than direct testimony, and who insisted on making these principles the basis of his advocacy of a Bill before a Committee of the Lords or Commons, or with another learned brother, who, in order to win his case, persistently put forth as true statements which he knew to be unfounded? Surely not! Why, then, should he grudge to phlebotomists and the dispensers of black draughts a freedom of professional action which he would claim—to use his own graceful style of designation—for Buzfuzes and the wearers of preposterous horsehair wigs?

But the claim of medical men to the free exercise of profcssional discretion is far stronger than that of barristers. Had Sir Edmund Beckett, in his anti-trade-union enthusiasm, agreed to have "with him" a learned brother hopelessly wrong-headed or unscrupulous, no great harm would have been done. Counsel on the opposite side would have exposed the ignorance or chicanery of his leader or junior as the case might be, the committee would have seen through it, and public interests would not have suffered. But at a medical consultation there are no counsel on the opposite side and no committee to appeal to, for the patient and his friends are intellectually and emotionally incapable of deciding on any difference of opinion that may arise. The doctors must agree as to the course of treatment to be adoptcd, and that quickly; and on their judgment the life of the patient may perhaps depend. How blameworthy, then, would that medical man be who, in view of responsibility, consented to consult and appear to co-operate with one from whom he radically differed, or in whose veracity he had no confidence! Should he do so, he must either stand by consenting to treatment he believes to be futile, assert his own views in defiance of his colleague and at the risk of his reputation, or lend himself to a gross deception. Self-respect and a sense of what he owes to those who seek his help should prevent any medical man from placing himself in such a position, and hence the refusal of medical men to meet with homœopathists is only a legitimate exercise of their professional discretion, conceived not more in their own interests than in those of their patients. Realising all this, genuine homœopathic practitioners ought to be as unwilling to consult with allopathic practitioners as the latter are to have any dealings with them. Why then, it may be asked, are homœopathic doctors, instead of consulting with each other, constantly seeking countenance and support from members of the orthodox branch of the profession?

Homœopathists probably differ very greatly among themselves in these days, and are of many shades of opinion, some of which vary

but slightly from the pure white light of medical science; but it is as impossible for an orthodox medical man to consult profitably with an out-and-out believer in the doctrine of infinitesimals and the *similia similibus curantur* theory, or with a man who, while professing homœopathy, is in the habit of treating all serious illness by large doses of drugs, as it would be for Lord Grimthorpe to take sweet counsel in astronomy (in which, if I mistake not, he has dabbled) with the gentleman who declares that this earth is flat (not in any figurative sense), and who has challenged the world, to the amount of £500, to prove the contrary, or to discuss ethics and keep his temper with a man who has grown fat on commissions. Homœopathists substitute for some of the recognised axioms of medical science axioms of their own. Every problem and proposition that can arise in the course of medical practice is affected by the change.

Lord Grimthorpe demands to know when, where, how, and by what authority homœopathy has been condemned? There have never been any œcumenical medical councils nor formal condemnations of medical heresies, but the estimation in which homœopathy is held by the medical profession may without difficulty be inferred from the attitude towards it which has been steadily maintained by the leaders of that profession almost without exception, and by the enormous majority of its rank and file, in declining to consult with those who have adopted it, and by the judgment passed on it in standard medical works. His lordship has allowed himself to be hoaxed as to the extent of the heresy. His allegation that homœopathists increase may or may not be true—fashionable follies have their ups and downs, and theosophy and high heels, it is said, have spread widely of late—but his assertion, on the faith of some pamphlet sent to him, that there are 10,000 homœopathic doctors in England is manifestly wild. I have not at present access to the figures, but I suppose there are in all about 22,000 registered medical practitioners in England, of whom probably 20,000 would emphatically pronounce homœopathy a delusion and a snare.

Lord Grimthorpe accuses the writers who have briefly answered one or two points in his special pleading for homœopathy as being "in a fury," whereas no one has spoken an irate word but himself. It is he who has lashed himself into that state in recalling his onslaughts on "clockmakers, architects, and even ladies," and in preparing himself for another attack on what he calls trade unionism. Now trade unionism is perhaps stronger than it ever was, notwithstanding all Lord Grimthorpe's tilting at it, and has a recognised place in our industrial policy; it is no longer a term of reproach; but to liken to it the attitude of the medical profession towards homœopathy is to abuse language and confuse thought. Medical men have never suggested that the smallest restriction should be put on homœopathic practice. They have desired that homœopathists should have freedom equal to their own, to practise, to experiment, to teach, to multiply, if it is in their power to do so, in all places. But from a sense of duty, and to their own pecuniary loss, individually and collectively, they have refused to consult with them, because, differing from them on fundamental principles, they have felt that they could not usefully deliberate with them on any particular case. It would be a strange sort of trade

unionism that would lead an artisan to say "I won't have this mate at my bench, for he'll spoil my work or scamp it."

The *odium medicum* and the *odium theologicum* have each their proper sphere of operation. Lord Grimthorpe would scarcely approve of the introduction of Mr. Bradlaugh to Convocation, but Mr. Bradlaugh would not be more out of place there than a homœopathist on a hospital staff composed of orthodox medical men. His lordship by implication admits that medical men are entitled to decline to have anything to do with disreputable or incompetent practitioners; he will perhaps permit them to define these classes for themselves. The fact is that there has been too little of the *odium medicum* in relation to homœopathy. Trusting to its inherent weakness and the good sense of the British public, the medical profession has treated it of late years with contemptuous indifference. But the aggressive tendencies which it has recently displayed and Lord Grimthorpe's ill-advised and intemperate advocacy will perhaps rouse the profession to expose once more, and once for all, to the popular gaze its fallacies and speciosities and absurdities. J. C. B.

DR. MILLICAN (2).

[*Jan.* 4.

It was not I who led this discussion into the merits of "Millican v. Sulivan," nor did I enter at all upon the merits of homœopathy as a system. The question I raised was the general one—"Has the homœopathic practitioner—not homœopathy—had fair play accorded to him as a duly qualified medical man?" I may be in error, but decidedly think not.

With the most ludicrous inability to see the drift of an argument, Mr. Fitzroy Benham ("The Founder" as he prefers to be styled—not an inapt pseudonym, by the way), Dr. Thudichum, and Dr. Frederick Pearse have entered the lists only to wander at once outside the arena in search of their antagonist.

Mr. Benham first remarks in *The Times* of the 28th inst. that he is not at liberty to say anything in reference to the action, and then in *The Globe* of the 29th proceeds not only to say something, but something which he had better have left unsaid—viz., that the grounds of my suspension were other than those alleged in his statement of defence, a copy of which document is now before me.

Dr. Thudichum has the next tilt at the windmills, and instead of replying to my arguments contents himself with taking shelter under that very claim of the medical profession to infallibility for which my letter in *The Times* of the 28th tried to extract some reasonable basis; and he amuses himself by adducing a lengthy quotation of a more or less mediæval period to prove what I never denied—viz., that ipecacuanha does not always cure vomiting.

Dr. Frederick Pearse is my present opponent. He, like Mr. Benham, wanders off into the case of "Millican v. Sulivan," but he does me the service of practically contradicting Mr. Benham. The latter gentleman has told us in *The Globe* of the 29th that "we" (the defendants) "were prepared to prove that the grievance was not only brought about by his joining the 'Margaret-street Infirmary,' but also by his actually practising homœopathy himself at the Queen's Jubilee

Hospital." As this point is the subject of documentary evidence, it needs no more than a plain denial from me. Now, on the other hand, Dr. Pearse tells us in *The Times* of to-day that, "unfortunately the matter at issue" (in the said case) "is quite outside the incompetency of homœopathy or its merits. It resolves itself into whether an institution favouring homœopathy is or is not likely to get the goodwill and support of the medical profession." What I would wish these gentlemen to settle between themselves is which of them is telling the truth.

Personal reflections so freely made use of by all three of my opponents, are beneath my notice. So far as the public are concerned, I leave them to judge between us from the course events have taken, while as for the profession to which my opponents so grandiloquently appeal, I am quite content to leave my professional character in the hands of those leading men in the profession who know me, and they are many, whatever may be their opinion of my views; while for my opponents' assumption that they are the spokesmen of the profession, I prefer to wait until I see how far the profession endorses them.

The most sensible letter of "the enemy" is that of their solicitor, Mr. Cronin, in to-day's issue, who very wisely does his best to keep his clients from inflicting further injury on their cause. I will so far assist him that having let these gentlemen display their own incapacity for logical reasoning, and so discount the value of any decision they may have "definitively spoken" on the subject of controversy, it is not my intention to trouble you with any further correspondence on this subject.

KENNETH MILLICAN.

LORD GRIMTHORPE (4).

[*Jan.* 6.

I have a truly allopathic dose of three columns of bad reasoning to work off, and I cannot treat it homœopathically, either in quantity or (I hope) *similia similibus* as to reasoning. The "Plebotomist's" specimen is the worst in every way except brevity, and for both reasons I will take it first.

He thinks it a clever and pleasant stroke of business to say I am "an example of the evils of irregular practice, and came to grief through making a client's will without the intervention of a solicitor —viz., for one Dent, a watchmaker." What I did for him I would do again for any equally intimate friend in the same circumstances; and the result would have been just the same if a solicitor had made his will. The intimacy—an important ingredient in the case—arose from my having managed the turret clock factory of him and his stepfather at their request, except financially, from the time the Westminster clock was ordered until his death, eight years after. I had also made a codicil, only just in time, for the elder Dent, when he was dying and wanted to alter his will and to leave one half of his business, including the clock factory, to F. Dent, instead of a small annuity, as he had done before through a misapprehension, which he repented of, as to his capacity for managing such a business. When F. Dent was dying, for some reason or other he would not let his solicitor be sent for, and begged me to make him a will to leave the ship compass busi-

ness to his foreman, who had really made it for them with some valuable inventions, and made the great clock under me; and also a few legacies. I wrote a rough draft for him, so far, with no disposition whatever of the residue of his property. The next day he showed me an executed will copied from my draft, and with a residuary bequest added in my favour, and he wanted me to take it away. I refused to take it, and left it on his bed. He died soon after, and I learnt afterwards that the will had been burnt. I, as the executor in it, must either defend it as a whole or give it up as a whole. I was perfectly certain that he had not changed his mind as to his foreman, and I had a great regard for him; so I defended it; but a British jury thought he had ordered it to be burnt. If his relations had told me they were willing to adopt the will except as to the residue, the case would have been very different, but they never did. I had then a much better business of my own, which I did not at all wish to give up, and the idea of my managing two shops—in the Strand and in the City—was absurd. There were doubtless some queer circumstances in the case which are not likely to be known, except to the actors, in this world; but that is no business of mine. I hope this "Plebotomist" is as well pleased as I am with the success of his little bit of spite, which he knew had no relation whatever to the question now in discussion.

"R. B. C.'s" letter is fitter for some homœopathist to answer than for me, who only know these things from what I may call instructions, in pamphlets and letters and some private information, and my own observations of the conduct of those whom he calls the "doctors" and the "orthodox." Let me further and more fairly abbreviate the two systems into "H" and "A." All these A's choose to assume that the question is whether they are justified in refusing to "meet" the H's in consultation. They know very well that that is not the question. You truly say that this very Jubilee quarrel is over the body of a man who was only condemned and deprived by them because he will not condemn somebody else elsewhere. I am not sure that even the Inquisition or Queen Mary burnt people for only associating with heretics while they were themselves orthodox. Still less is it the question whether H is a better or worse mode of curing or killing than A; and I shall not discuss whether it is a right or wrong "principle and guiding light of modern medicine" to try and cure a cough as soon as you can, as the H's do, or to wait with the A's till you have found out its origin, or think you have, and then try what you can do. I am utterly unprincipled, and I am glad to see that Darwin had the same "suspicion of *à priori* principles" as I have long avowed. When I have a cough I want it cured without doing me any other harm, and I do not care the least on what principle the doctor discovered or guessed at the proper medicine, even if it was "his own immoral consciousness," as "R. B.C." absurdly says of Hahnemann. It is "the province of the imagination in science," as Tyndall calls it, to guess at the right experiments or the right theories to try. When they have been tried and succeeded, we have no more to do with their origin. But it is by no means so easy to prove the impossibility of the principle as these denouncers of it assert, even independently of the results of homœopathy.

The last time I was vaccinated—a purely homœopathic proceeding—

the operator, of the most orthodox persuasion, beguiled the time by remarking on the wonderfully small quality of actual vaccine poison that does the business. I said, "Yes, and why should not an equally small quantity of homœopathic medicine?" He smiled, and shut up his lancet and himself. May I ask "R. B. C.," too, who expatiates on the "decillionths," which he knows they have largely increased, how big he thinks the spores are which we are gradually being told cause nearly all our diseases, or how much poison of a tsetze fly really goes into a horse or ox that dies of it.

He says I "shall perhaps be surprised to learn that, according to the current report among druggists (who mostly live by allopaths), the homœopathic pillules sold to old ladies (and therefore to everybody), do not contain a particle of the various medicaments after which they are named," but only sugar and such like things. I am not the least surprised at any such report being circulated in A interests. But I have heard another, and one which neither my informant nor his authority for it, one of the principal London druggists, had any interest in inventing, viz., that nobody (except the A druggists) has any idea of the quantity of H medicine that is secretly prescribed by A doctors. If an H had told me that, of course it must have been a lie, for we know from what was sworn in the Jubilee suit, and I know from some A letters I have been receiving, that "every H is an impostor and a liar, and (in Jubilistic grammar) a fraud."

By way of answer to my question, what excuse the A's have to give for making the committees of two hospitals refuse those two offers to found and maintain experimental H wards, "R. B. C." tells us the uselessness of it was proved above fifty years ago in France "thus wise":—A certain French doctor, Andral, in 1835 "said he was decidedly opposed to allowing a homœopathic dispensary. He had given homœopathy a fair trial himself. He had taken homœopathic quinine, but had contracted no fever; aconite, without being affected with the symptoms of plethora; sulphur, without catching the itch; and so on with various other homœopathic substances, and had always failed in curing patients homœopathically." I have only shortened his language a little. The last line of it is the only one of the smallest practical significance. The absurd unfairness, and evident dishonesty of Andral's experiments has been several times shown up. I refer anybody who wants to see the exposure to "Sampson's Homœopathy," third edition, 1850. And that one 52-year old assertion of an enemy is all the excuse "R. B. C." has to give; and then he calls it such "abundant evidence" of the fallacy and fraud of H as ought to prevent any hospital committee from ever allowing the experiment to be tried by those who offered to pay for it being tried fairly. But, unluckily for him, that very thing has been done, not once, but a good many times since 1835—unless the H pamphlets all lie too. Here are a few specimens, which I will give as briefly as possible, from one of them, No. 5. For three years about 1851 half of one hospital in Paris was put under a homœopathist, and the result was that 8·5 per cent. of his patients died, while 11·3 per cent. died under the allopaths; and the H cases were the most numerous. The H doctor had no power of selecting his patients. In a Vienna hospital, 5·7 per cent. died of pneumonia under H and 25·5 under A; of

pleurisy, 3 per cent. under H, and 15 under A; of peritonitis, 8 per cent. under H, and 13 under A. A certain orthodox Dr. Routh tried to explain away some still stronger results at Leipsic, and guessed that those at Vienna were not fair average cases; but Sir W. Wilde, of Dublin, who had personally inspected the H hospital, wrote that they were quite as acute cases as he had seen elsewhere.

I had long heard that H was peculiarly successful with cholera, and the same pamphlet shows that in several epidemics of that kind it had cured from twice to four times as large a proportion as A. In 1854 it seems that the English Government had the courage and power to order the general patients to be cleared out of the H hospital of London to take in cholera ones, under its own doctors, and appointed a large Medical Council, all of whom were "regulars," to report on the results from all the hospitals, and medical inspectors were also appointed. That council presented the reports from all of them except the H hospital, though the inspector had made his report of it to them. At last the House of Commons ordered them to report that; and then they had to confess, with disgust, if not with shame, that the deaths there had been only 16·4 per cent., while the average at the others was 51·8 of the patients. And what made it worse was that I see that the President of the Board of Health, who appointed the Council, expressly said that the object was to "determine the effects of the different systems of treatment;" which they quickly ignored in their report, and expatiated in their favourite "pathology" instead. It is quite certain that if cholera came again, the H treatment, which had been far more successful than all the different A nostrums which they detailed, would be excluded again from every hospital and every Poor Law Infirmary in the kingdom, and everybody would be denounced as a manslaughterer at least who gave a H dose and did not cure his patient, unless at last *mundus surrigat justis furiis* against these worse than manslaughterers; for they persist in forcing what has been proved to be destructive rather than healing medicines on their patients, and excluding the healing ones. What has "R. B. C.," with his "abundance of experience," and "immoral consciousness of H's," to say to all that, either physically or morally? It would be tedious to give more such extracts, and I will only add that in a New York H hospital the mortality of children during twelve years was one in 146, and in the average of the others one in 41. Many people have told me that they find H peculiarly successful with children, and I am more and more struck with the number of houses that keep boxes of (doubtless only bread and sugar) pillules for the children mainly, but also for "grown-ups;" which is a very material fact in estimating the advance of H. I now learn that there are no less than 11,000 H practitioners in America, or more than there were in all the world a few years ago, according to the pamphlet which I quoted from before.

But see what a number of A's and how few (if any) H's have got into the Royal Society, says "R. B. C." Is he really ignorant how everybody else laughs at the way they push in there to advertise F.R.S. after their M.D.? And precious little chance any H doctor would have there of not being "pilled" by them—in the clubbicular, not the medical sense; as it was well known that they profusely did

to a very popular H doctor some years ago at the Athenæum : another proof of the falseness of the pretence, that they object to meet homœopathists in consultation.

The only thing worth notice in " J. C. B.'s " letter, as distinct from " R. B. C.'s " (for his silly attempts at some more imaginary analogies at the Bar are not), is that he at last gives an answer to my repeated question, What formal condemnation, and (if any) by what authority, has ever been officially launched against H ? He admits, none. Not that it would be of any scientific or moral value if they had ventured on any, in the face of such a body of heretics as now exist. It is true however that, individually, many great men among the orthodox have been very unreserved in their own assemblies, and have used the very strongest language about the " backward and unsatisfactory condition of the art ; " " such infamous medical treatment in hospitals that " the greatest surgeon of the century said he " could not bear to witness it ; " that " in a large proportion of the cases treated by (a certain class of) physicians, the disease is cured by nature, and not by them, and in a less, but not small, proportion, in spite of them ; " as I quoted before in substance from Sir B. Brodie's article in *The Quarterly*—that " the leaders of medicine, both here and abroad, are sceptical of the curative influence of (a certain class of) drugs on disease ; " and the same distinguished doctor adds that " anyone of his school who met a H. practitioner would be guilty of an immoral act." Another equally eminent and violently anti-homœopathic Scotch doctor said : " There has been no want of new remedies of empirical origin introduced in the last forty years, of which some have stood the test of time, but most have been mere rubbish ; " another that " medicines are given at random with no defined idea of what they should do, and trusting to chance ; " and a great deal more to the same effect, and often in the same words, especially as to nature—their favourite argument—obviously adopted from one doctor by another. I count more than thirty of such denunciations given in No. 9 of the " Homœopathic League Tracts," (which ought to be dated, but are not.)

You may say, " Well, what then ? We all know by this time what the A's think and say of the H's," and naturally they speak still more freely when they are " tiled," as I believe freemasons say, in their own assemblies. Yes, but now I have a small secret to let out. You will hardly believe it, but every one of those hard sayings was uttered, not about homœopathy, but about the orthodox and every-day practice and system of the very men who uttered them. The latest of them, Dr. Moxon, finished a speech, too long to give more of, thus : " As to medical progress, there is no such thing. We guess our way, and call the guesses theories to make them respectable. Those minds that are shaken by sickness suppose that we can cure them, and we encourage the supposition." Such are the domestic confessions of those who go about thundering against homœopathic lies and sugar pills and trusting to nature.

There I will leave them—for the present, at any rate. We have read of popes and priests who were suspected, if not known, to be infidels and yet persecuted heretics as briskly as the best ; but they kept the secret of their unbelief better than these successors of them do.

All the subscription hospitals are evidently approaching a revolution, which will end either in ruin or in support by rates or by patients, which means external government. And that will no more stand this medical tyranny than Parliament would in 1858, when it inserted the 23rd and 52nd sections in the Act, expressly because some of the licensing and degree-giving bodies had begun to demand pledges not to practise homœopathy. Their pretence now—that they only want to leave the H's alone, and not to meet them in consultation—is obviously false. In every possible way they have shown their determination to expel and ruin them in every place where the A's have got dominion, though it is evident that the best of them are private unbelievers in their own avowed faith. The H's are not that, at any rate.

GRIMTHORPE.

DR. R. E. DUDGEON.

[*Jan.* 6.

Your correspondent "R. B. C.," whom we recognise as our old antagonist in the controversy that took place at the period of Lord Beaconsfield's last illness, makes, as before, a number of assertions respecting homœopathy which are not in accordance with the well-known facts. It is not a fact that Hahnemann's first edition of the "Organon," published in 1810, "was not the fruit of study or research, but was evolved by the inventor out of his moral (or immoral) consciousness." On the contrary, Hahnemann's first essay on the homœopathic therapeutic rule was published in 1796 in *Hufeland's Journal*, and it was after fourteen years of patient study and self-denying experiment that he gave to the world his perfected views on the homœopathic practice in his great work, the "Organon of Rational Medicine." It is not the case that the method of treatment advocated in this work rested on the *psora* doctrine. This doctrine of the origin of some chronic diseases (not, as "R. B. C." says, of "all diseases") from a species of miasmatic virus, shown by various eruptions on the skin, was not peculiar to Hahnemann, but was originally suggested by Autenrieth, and only adopted by Hahnemann with modifications in 1828. It is not the case that Hahnemann did not know that a minute insect was the cause of itch. In 1792 he wrote an article in a popular scientific periodical, and in 1795 another article in a medical periodical, in which he states that itch is caused by a minute insect, and he gives directions for killing it, and thereby curing the disease. It is not the case that those medical men who follow the homœopathic rule in treatment neglect pathology. On the contrary, several professors of pathology in Universities have been practitioners of homœopathy. I may mention Henderson, of Edinburgh, D'Amador, of Montpelier, Rapp of Tübingen, Arnold of Zurich, and Zlatarovich, of the Josephinum Academy in Vienna.

It is not the case that any official trial of homœopathy has been made in Paris. Andral's pretended trials were shown at the time to be not only not homœopathic, but contrary to all the principles of that method. Bally's permission to Curio and Simon was accompanied by all sorts of obstacles and violations of the conditions agreed upon, and the results were never published, for Bally always pretended that

the day-books of the cases treated, which he carried off, were lost. It is not the case that the homœopathic chemists supply their customers with unmedicated pilules, and "R. B. C." should not bring what he calls a "common report" as an accusation against the honesty of a highly respectable class of tradesmen. Your columns are not the proper place for a discussion on the principles and practice of a medical system. The medical societies and periodicals are the proper places for that; but, unfortunately, we are boycotted so effectually from these that we are forced to have societies and periodicals and hospitals of our own.

Your correspondent "J. C. B." seems to think we are longing for consultations with our allopathic brethren. This is not the case. Occasionally a patient or his friends desire an old-school authority to be called in to determine the nature of some obscure disease. We should have no objection, but so effectually is the boycott against us carried out that we are aware that few physicians would dare to meet us for the purpose of such a consultation. As for desiring the opinion of any old-school authority on the medical treatment of any disease, that could never be, as, having practised both, we know the new system to be in every way superior to the old. We sometimes require the assistance of an operating surgeon, and I am glad to say the best of them are always ready to give us their best aid, though I have met with refusals from some narrow-minded surgeons who seem to have forgotten the prime duty of their profession, which is to give their aid to their suffering fellow creatures.

If "R. B. C." has seen so little of his colleagues who have adopted the homœopathic rule of practice, that shows how completely the boycott system has been carried out. If our colleagues of the anti-homœopathic persuasion will admit us to their societies, I will answer for it that they will soon both see and hear enough of us. All we want, all we ask, is fair play and free discussion. Do our orthodox colleagues dread these things? If fair play and free discussion in the medical societies were allowed, we should hardly find partisans like "R. B. C." entertaining such grotesquely erroneous views respecting homœopathy, and controversialists like "J. C. B." impugning the honesty of colleagues who differ from him for the sake of discrediting their opinions.

Homœopathists are indebted to you for the calm judicial manner in which you treat the subject in your leader of to-day. On one point only I could presume to correct you, and that is that we represent Hahnemann as having introduced a principle or rule of treatment hitherto unheard of. On the contrary, Hahnemann himself traced the homœopathic rule in medicine through all ages, from Hippocrates downwards, and we have always alleged that it was no novelty in medicine. Hahnemann's merit was that he showed it to be a rule of more general application than his predecessors had considered it.

I shall not weary your readers by refuting all the errors of "R. B. C." I would only say here that the astronomical calculations of Mr. Smith are quite beside the question, as only about six ounces of alcohol are required to make the so-called decillionth dilution.

R. E. DUDGEON, M.D.

53, *Montagu-square.*

Permit me, as one of the so-called heretics in the profession, to thank you for your most able and judicially fair article in your issue of to-day. You state our principle perfectly correctly, when you say that "homœopaths of this school" (all homœopaths say the same) "maintain that the essence of their system lies in the therapeutic rule that the drug most likely to remove given symptoms is the one which most exactly simulates them when administered to a healthy person." Whoever thus prescribes is practising homœopathy. Your next sentence puts the whole question in a nutshell:—"This rule may be sound or not, but at all events it brings the whole matter to the empirical test, which, with all deference to medical science, is always the ultimate and conclusive test." However beautiful a therapeutic law or guide may be, it is valueless unless it stand the test of practical results. We have been preaching this doctrine year after year, and what we ask and claim as a debt to mankind is that our system should be put to the test by each individual for himself. It is the practical results which draw our hospital and dispensary patients to come for our treatment, when they know nothing of the principles of homœopathy. It is a curious fact, but fact it is, that no practitioner who has first made himself *au fait* with the right use of the medicines on the principle of similars, and tested them in practise honestly, fails to be convinced. Several of our best men have been converted in spite of their desire and hope that they would find the whole thing a mistake. An illustration of this was the late Dr. Horner, of Hull. He was President of the British Medical Association, and was asked to write a book against homœopathy. While agreeing to do so, like a sensible man, he first resolved to study what he was to write about. The result was that he wrote a book in favour of homœopathy, and from that time till his death was a stanch adherent and practitioner of the new system. For this piece of honest work he was removed from his post as one of the heads of the Association. Let us, then, have this "ultimate and conclusive test," and if honestly carried out the result is foregone. You also state that "It (homœopathy) holds that dosage is not an essential of doctrine at all, but a matter of practice and experience." This, read along with a former sentence, is perfectly correct. "The reason assigned for attenuation is that if medicines are carefully selected according to the homœopathic rule, and given for the cure of symptoms similar to those which they produce in the healthy body, it becomes absolutely necessary to give very small doses, otherwise aggravation instead of amelioration is the result." This is the accurately stated rule of the dose. It must be less than will aggravate the symptoms. How much less is the best and most successful dose is purely a matter of experiment and experience. And, provided the medicine is selected according to the rule of similars, he who cures with a comparatively large dose is equally practising homœopathy with his brother who finds he can cure best with the higher dilutions. And here let me state that, taking experience as the test and guide, infinitesimal doses cannot be given up. Their value is only elicited by results, and those results are such that no practitioner who has seen them in his own practice would agree to give up their use. One frequently finds that, owing to the

varying sensitiveness to medicines of different constitutions, the right medicine fails in a "tangible" dose, and at once benefits when altered to a high dilution, and *vice versâ*. We therefore claim it as our right and our duty to use the whole scale of dilutions in accordance with our judgment and experience. Even the decillionth (the thirtieth dilution), in which, as you correctly state, "the division of matter involved becomes simply unthinkable," is in many cases an attenuation which we could not do without. Here, again, it is a question of experience, and not of preconceived theory. Did we give way to our prejudices, we should never dream of using such highly attenuated drugs, but theory must go to the wall before results. The pharmaceutical problem of preparing this dilution, which one of your correspondents, *à la* Sir James Simpson, tries to ridicule as an impossibility, is solved by every homœopathic chemist, with the aid of 30 phials, 6 oz. of spirits, and half an hour's time. Taking one drop of the pure tincture, adding 99 drops of spirit; then taking one drop of this solution and again adding 99 drops of spirit and so on, brings one to the thirtieth dilution, or the decillionth of the pure drug, in a very short time. You also most clearly and correctly state the mode of selection or differentiation of a homœopathic drug in a given disease, taking as an illustration the use of grey powder in diarrhœa. As you truly say, "It is fair, however, to remember that homœopaths repudiate the idea that they go by one symptom. There are a hundred things that produce diarrhœa, and in choosing grey powder for a given case they are guided by the concurrence of many collateral symptoms that follow the ingestion of grey powder in a healthy subject." Exactly so. To use our phrase, we select a medicine which "covers the totality" of the symptoms. And here let me repudiate, in the name of my *confrères*, the often-repeated charge which "R. B. C." brings up again. He says that homœopaths ignore pathology altogether, "his patient coughs, and all he has to do is to look in one of his books to find out what medicine will produce cough in a healthy person, and then to administer this medicine," while the "orthodox" practitioner makes it his "business to find out why he has a cough, and then to apply the appropriate means for his relief." Two vastly different processes. Were "R. B. C." not in lamentable ignorance of homœopathic practice he could not thus speak. Homœopaths are as well versed in pathology and physiology as their brethren of the old school, and cannot value them too highly, in their place, as essential to their knowledge of disease. They examine their patients by every known mode, and thus can say why the patient coughs, and can diagnose the disease present. But when it comes to the treatment, they refuse to be guided by pathology, knowing well that the trust in pathology as a guide to treatment has been the bane of medicine from the earliest days, and has been the cause of the continually shifting practice of the old school. Homœopaths maintain that, given the pathology and diagnosis of a disease, each case manifests itself to the eye of the physician by its symptoms only, many so-called diseases being mere symptoms of a deeper mischief. Under the head of symptoms they include objective as well as subjective symptoms. They maintain that, in treatment, theory of diseases ought to be discarded and the

symptoms present taken as the only guide to the selection of the medicine. Nor, we maintain, is the occurrence of similar symptoms in a given disease and a given drug a mere superficial similarity. It stands to reason that a drug which can produce an exact picture in the healthy body of a given disease, must have some deep and inner relation to the parts involved of a more than superficial character, and that such a drug, if prescribed, must go to the seat of the malady, and influence it in a very special way, for good or ill. We thus maintain that our mode of selecting the medicine is far more sure and scientific than any mere theoretical selection. And it will be seen that, in treating a patient, the only point in the joint path where the two schools diverge is the drug-treatment. Diet, general hygienic means, &c., are the property of no school, but of the whole profession.

You quote Dr. Ringer as an authority for giving ipecacuanha in sickness; but, may I ask, where did Dr. Ringer get his information on this and many other bits of "new" practice? Is it a mere coincidence that, till Dr. Ringer's book was published, the using of small doses of ipecacuanha in sickness, of minute doses of corrosive sublimate in diarrhœa and dysentery, of arsenic in diarrhœa and gastritis, of cantharis in inflammation of the kidneys and bladder, and many other bits of treatment, was not only unknown in the old school, but was directly at variance with the orthodox views in medicine; while these very pieces of practice were in every-day use among homœopaths and were to be found in every work on homœopathy? The only journal at the time which saw, or thought it desirable to see, the drift of this mass of new treatment was *The British and Foreign Medico-Chirurgical Review*, which, coming out quarterly, and long after the other favourable reviews, said:—"This is neither more nor less than pure homœopathy." We maintain that the popularity of the new treatment and the gradually increasing adoption of homœopathic remedies by the old school (*vide* the "Index of Diseases" in the recent work of Dr. Lauder Brunton, physician to St. Bartholomew's and examiner in therapeutics at the Royal College of Physicians) is ample proof how the wind is blowing, and that it is only a question of time to see the homœopathic method the predominant one.

I apologise for the length of this letter. I must leave the many misstatements of "R. B. C." to be dealt with by others of my brethren, preferring to abide by your excellent advice—"It is better worth while to inquire by what side homœopathy appeals to men of average probity and intelligence than to draw extreme deductions from premisses possibly but partially apprehended, and then to brand all homœopaths as either knaves or fools."

I would only commend to the personal consideration of "R. B. C." his own sentence—"There is, of course, no limit to the erroneous opinions which people may entertain about subjects of which they are profoundly ignorant." If "R. B. C." had applied this to himself he would not have written as he has done, showing to all who know what homœopathy is that he has "but(very)partially apprehended" the subject.

As to the disgusting personal charges of dishonesty put forward by "R. B. C." and "J. C. B.," they are beneath notice, especially after your own remarks. One is only surprised to find that such charges, which we thought had been dropped for ever, could at this time of day be brought up again. *Honi soit qui mal y pense.*

29, *Seymour-street, W.* D. DYCE BROWN.

DR. MILLICAN (3).

[*Jan.* 6.

Against my will, I find myself once more drawn into the whirlpool of controversy, for the letters of "R. B. C." and "J. C. B.," from the professional standing of their writers, demand a reply from me after the line I have hitherto taken. The main points of their arguments were, I think, replied to in anticipation by my letter in your columns of the 28th ult. But "J. C. B." summarises the scientific objection to professional intercourse with homœopaths—which, as I have shown, has been practically discarded in favour of the more practical ethical one by Drs. Lauder Brunton, Austin Flint, *The Lancet*, and other authorities—when he says, "Orthodox medical men refuse to meet homœopathists professionally, I take it, . . . because they hold views which render conjoint deliberation impossible, or profess principles which they do not honestly believe."

Now, once for all. If there be one drug which can and does check a morbid symptom which the drug is itself capable of producing, then there is no impossibility in the "law of similars." I think I have conclusively proved such to be the case with ipecacuanha. Now, what happens in one instance may possibly happen in others, and whether it does or does not is merely a question of experience. But inasmuch as diversity of experience on other subjects—*e.g.*, Listerism—does not prevent the holders of opposing views from meeting one another in consultation, there can be no reason beyond caprice or prejudice—*i.e.*, no valid reason—why it should do so in this instance. Of course, every man has individual right of refusal to meet homœopaths if he pleases, or, indeed, any member of his own profession, just as it is within my rights to refuse to speak to a man with a "billycock" hat when I wear a "chimney-pot." But if I call upon all others who affect the latter wondrous headgear to do likewise—nay, even if 90 per cent. of the hat-wearing population joined me in my demand—the request would be arbitrary, tyrannical, and morally beyond justification.

As to the alternative imputation of dishonesty, that is completely answered by the same argument. That which has occurred in one instance may possibly occur in others; and to accuse a man of professing "principles he does not honestly believe," simply because his experience differs from your own, is a mode of controversy which, when translated into its synonymous expression, "you're a liar," is usually ruled out of order, except, perhaps, in Parliament.

I write with all due respect for "R. B. C.," as will be shown by the admission I here gladly make of my indebtedness to him on more than one occasion for kindness and acts of professional courtesy. But I really must take entire exception to his view that the "fundamental law of homœopathy"—the so-called "law of similars"—is "directly at variance" with the principle "which is the guiding light of modern medicine"—viz., "that it is the business of the physician to go behind symptoms to the morbid changes which are underlying them." In my opinion such a conclusion is a *non sequitur*.

Let us take cough, the instance adduced. How are we to distinguish between the different sources of coughs, and "go behind the symptoms to the morbid changes which are underlying them," excep

by studying the respective characters of the coughs? Take, for instance, the "throat cough" to which "R. B. C." refers. Its character is a sharp, dry, explosive bark, and its underlying condition is irritation—*i.e.*, disturbance of equilibrium of a special nerve supplied to a particular portion of the throat. Now, assuming that there is a drug capable of reproducing a cough with exactly those characters, it is a fair inference that that drug does so by acting especially upon the particular parts concerned. We are not, therefore, rejecting the underlying morbid change—that is the cause of the cough—but merely omitting a repetition of intermediary proved facts in a manner familiar to every schoolboy through our friend Euclid.

As regards the trials of homœopathy in France, I have only to remark that I have heard—though I do not recollect details—that the trials took place under most unfair conditions. In Bally's case, it is urged, only incurable cases were given for experiment; in Andral's that he was absolutely ignorant of the system he was trying. Under any circumstances, as I am not a partisan trying to prove a case either for or against the homœopaths, but merely pleading for fairness, I think it only in accordance with "natural justice" to withhold my opinion on those facts until I hear a reply, which I trust will be forthcoming. One thing, however, strikes me. "R. B. C." quotes Andral as stating that he had treated "about 130 or 140 patients" homœopathically. Now I do not know if those are "R. B. C.'s" words or Andral's. But if the latter, I should say that such an utter indifference to the necessity for accuracy would make me seriously discount the value of any evidence he might offer in regard to his treatment of those cases.

In reference to the number of homœopaths, of course, so long as they have no chance of obtaining hospital appointments, of joining the medical societies, or of mixing in professional intercourse with their *confrères*, either on public occasions or at private consultations, they are scarcely likely to be much heard of. Birds of a feather flock together. The Bohemian whose associates are all of that ilk is quite amazed when he finds some new acquaintance objects to lawn tennis on Sundays or is shocked at a lady who smokes; while the respectable member of country society who is taken up "the river" one summer Sunday afternoon is aghast to find himself surrounded with crowds of people, apparently unconscious that they are doing anything unusual, who ignore every conventional rule which he has been accustomed to see regarded on all hands as beyond question.

Finally, "R. B. C." knows me and I him. But the vast majority of your readers probably do not pierce, as his *confrères* do, the veil of his identity. Now my name was made public property in this controversy through the action referred to by Lord Grimthorpe in his letter on the 24th ult. That being so, I would ask "R. B. C.," does he think his final insinuation (especially when made *incognito*), that I sign my name out of desire for advertisement, either just or generous? I think his well-known professional uprightness and courtesy justify me in assuming that the last lines of his letter, at least, were written without due reflection.

KENNETH MILLICAN.

PATERFAMILIAS ON HOMŒOPATHY.

[*Jan.* 6.

Medical men, who have vested interests, should not be allowed to have a monopoly of the interesting discussion raised in your columns, and I hope you will allow a father of a family, who owes a great deal to what is called homœopathy, to say a few words in its favour.

I have not a word to say in defence of the fanatical extravagance which characterised the foundation of the homœopathic system, though as an extreme rebellion against the barbarous old system of the orthodox school of medicine it had its uses, just as the absurdities of pre-Raphaelitism had in making painting more true to nature. But the letters of "R. B. C." and "J. C. B." are directed against extinct absurdities, though I admit that they are right in condemning homœopaths for not honestly repudiating extravagances which are now neither believed in nor practised. But this should not prevent people from feeling grateful to homœopaths for the great reform in medicine which their attitude and the introduction of their excellent tinctures have produced. From the success of homœopathy the allopaths have been shamed out of their barbarous system of violent dosing, "blooding" and weakening formerly practised.

But parents have most reason to be grateful to homœopaths for the introduction of an easy and simple system of treating the minor ailments of themselves and their children. It is a great thing to have tasteless tinctures in place of the abominably nasty drugs that I had to take in my childhood. For an aperient, instead of a disgusting dose of rhubarb or salts and senna, I can now take two or three drops of tasteless bryonia; while for an inactive liver, in place of a violently purging "blue pill," I can put myself right with a little mercurius or hydrastis tincture. The great benefit of these mild medicines—mild in the small but not infinitesimal doses in which they are administered—is that they may be taken at the very beginning of a disorder, before one would dream of going to a doctor. Thus many serious attacks are staved off.

In my childhood I nearly died of croup more than once, and I was frequently attacked by it. Treated by an allopath, I was dosed with antimony wine as an emetic; but he had nothing to give me to check the first symptoms of croup—the croupy cough. Each of my five children has been subject to croup, but not one has had a dangerous attack, simply because aconitum or spongia has always been administered when the first symptoms—the croupy sound in a cough—was manifested.

One great cause of the anger of allopaths against homœopaths is that the latter have "let out" professional secrets, and so spoilt trade. The former would never have enabled me to keep a number of simple remedies in the house, with instructions for their immediate use. An intelligent man can deal with many minor disturbances of his system as well as some chronic ailments, when he once knows the medicine that suits him, as well as a doctor. At any rate he can do better by using remedies which have benefited him before than by waiting till he is seriously ill and then calling in his medical man. Few people can afford the heavy bills which doctors now send in. If called in for ever so slight an ailment the doctor makes several calls, and usually

charges at least 7s. 6d. a visit. Why, when gold has appreciated and nearly everything else is much cheaper than it was, medical attention should have risen greatly in cost I cannot tell; but this is an additional reason for gratitude to homœopathy, which enables thousands of people to save heavy doctors' bills.

PATERFAMILIAS.

MEISSONIER'S TESTIMONY.

[*Jan.* 6.

In the present controversy on this subject, you may, perhaps, in your spirit of fairness, think the enclosed document worth publishing. It has been in my possession for some time.

I was studying painting a few years ago with Meissonier, whose valuable dog—which had been given to him by his great friend Dumas—was struck with paralysis in its hind quarters; it had also its neck twisted.

I had long studied homœopathy for my own use, and my little globules were the subject of much good-humoured fun to Meissonier and his friends and family, who did not believe in them at all.

The dog in question was condemned to death by a great "vet." in Paris, who attended to Meissonier's very valuable horses, as will be seen in the enclosed testimony. The same evening I was dining with him and his family, and the dog was in the room—a subject of much lamentation—when, in his sudden and animated manner, he challenged me to cure it with "my homœopathy."

I accepted the challenge, and gave the dog at once in their presence a single dose of Rhus Tox, of a rather high dilution.

The next morning I was at work with him alone in his garden studio before breakfast, when his clever and energetic daughter came rushing into the studio as if the house were on fire, crying out that "the dog walked."

We ran out of the studio—Meissonier with his brush in his mouth and his large palette on his thumb, in his earnest eagerness about everything that freshly caught his attention—and there was the animal running about on its four legs as strongly as ever.

It still had its neck twisted, however, and I was much puzzled to know how to proceed with my patient. I then perceived that its coat was rough and staring. Here came in one of the great principles of homœopathy—that every symptom must be taken into account—and the proper remedy at once suggested itself. I gave it two doses of Arsenicum, 3 X.; the dog quite recovered, and is, I believe, alive and well to this day.

A PUPIL OF MEISSONIER.

"Messieurs Meissonier père et fils apprenant qu'on met en doute le guérison d'une petite chienne condamnée à mort par les vétérinaires de Paris, affirment que cette petite bête été radicalement guérie d'une ettaque de paralysie extrêmement violente par leur ami ——— ———, qui l'a traitée par l'homœopathie. Cette petite chienne de race tres-pure va parfaitement bien et fait l'admiration de tous.

"E. Meissonier.

"Ch. Meissonier, fils."

THE FACTS OF "R. B. C."

[*Jan.* 6.

Of the statements in "R. B. C.'s" long letter, which you published yesterday, there is only one that I can check by my own knowledge; and that one is incorrect. In his enumeration of the members of the medical profession who have been in the forefront among the cultivators of pure science, "R. B. C." says "both the Darwins were doctors." Obviously the phrase "both the Darwins" is intended to include Charles Darwin, whose life we have all been reading. But Charles Darwin was not a doctor. At the age of sixteen he was sent to Edinburgh University to enter upon medical studies, but he found them so distasteful that he abandoned them, and afterwards thought of going into the Church ("Life and Letters of Charles Darwin," vol. i., pp. 36—45).

I conclude from this that "R. B. C.'s" facts cannot be safely relied upon until they have been verified, in spite of his authoritative tone.

LAYMAN.

R. B. C. (2).

[*Jan.* 10.

In the letter on homœopathy which you did me the favour to publish on the 4th inst., my object was merely "to explain, and to endeavour to justify, the attitude of the medical profession towards homœopathic practitioners." I had no wish to engage in a controversy with homœopaths, and must decline to do so as far as any question which requires medical knowledge for its determination is concerned. Neither the arena nor the audience is fitted for such a purpose. I will, nevertheless, with your permission, refer briefly to certain points in the replies which my letter has called forth; and will also offer some remarks upon a hitherto untouched aspect of the main question.

I must first penitently acknowledge, and thank "A Layman" for pointing out, my error with regard to Charles Darwin, who appears only to have commenced an education which I thought he had completed. I might, of course, have taken many medical philosophers for the purpose of the illustration; and, as the name was a matter of detail, not affecting the argument, I neglected to verify the impression under which my hastily written letter was composed.

Lord Grimthorpe is very hard upon me. He not only describes me (I am glad to say, in excellent company) as a "bad reasoner," and as "absurd," but he attributes to me, by placing it between inverted commas as a quotation, the word "orthodox," which my letter did not contain. Fortunately, his lordship diminishes the force of his denunciations by admitting that he is not acquainted with the subject on which he writes, as well as by furnishing the most unequivocal proofs that this is really so, and that the admission is not the mere *façon de parler* of an ingenuous modesty. He absolutely compares vaccination to homœopathic medication. Now, the former neither produces smallpox nor cures it, and the efficacy of vaccine virus, as of some analogous products, is due to the presence of living elements, which increase the dose by rapidly multiplying within the body. I have not heard the presence of such elements claimed in the case of any medicine, and it would have to be demonstrated, as well as claimed, before any

analogy with vaccination could be admitted. Moreover, the vaccine virus, as well as the poison of a snake or of a wasp, and also, I presume, of a tsetze fly, is inserted in a visible and appreciable quantity, which bears no relation to the small doses of homœopathy.

Lord Grimthorpe tells us that homœopathic medicine is "secretly prescribed" by doctors. May I ask how that is possible? A prescription is a written document which cannot be "secret," which is usually preserved by the patient, and always, in the form of a copy, by the dispenser. Again, the phrase "homœopathic medicine" has no particular meaning. A medicine may be described as "homœopathic" on one of two grounds—either because it is given in what is commonly called a homœopathic dose, or because it has been selected for the patient on homœopathic principles. No medicine is "homœopathic" by reason of any inherent quality which it possesses, and there is no medicine which I should hesitate to prescribe if I thought it would be useful to my patient. If Lord Grimthorpe means that doctors prescribe in homœopathic doses, or on homœopathic principles, I shall deny the truth of the assertion until he has produced the prescription in evidence.

I do not think I need defend the Council of the Royal Society against Lord Grimthorpe, nor that I need follow him through the statistics which he has culled, as I gather, from homœopathic pamphlets intended for popular circulation. Nor need I do more than point out that his objection to Andral's experiments, merely on the ground that they were conducted fifty-two years ago, would condemn us, if it were valid, to a perpetual re-opening of settled questions. I may say, for the information of Mr. Millican, that the account which I gave was quoted verbatim from an English contemporary translation of the minutes of the Academy, but the word "about" was a misprint for "above." The actual report of the committee would probably give the precise number of patients, and I confess it struck me as an evidence of Andral's exactitude that, being possibly doubtful as to one or two, he did not give a precise number in his verbal communication. It is at least certain that the experiments were satisfactory to the members of the Academy, whose decision is reported to have been unanimous; and it is inconceivable, if the Parisian homœopaths of that day could have urged any plausible objection against what had been done, that they would have failed to urge it at the time, and in arrest of the judgment which was delivered against them. The Academy must have contained many members who would have shrunk from incurring any suspicion of unfairness.

I have been reproached both by Lord Grimthorpe and by Dr. Dudgeon, for repeating the common belief that homœopathic globules (and, I may add, tinctures) do not contain, in their so-called higher potencies, any of the several medicines after which they are respectively named. I will speak presently of the evidence, but would first invite attention to the inquiry whether the supposed "attenuations" are possible. The answer manifestly depends upon the number of portions or pieces into which a grain of solid or a drop of fluid is physically divisible.

Perhaps the finest division of solids known to pharmacy is that which exists in "Dover's Powder." This preparation consists of one

part of solid opium, one part of ipecacuanha, and eight parts of sulphate of potash. The sulphate is in the form of very hard, sharp crystals, which are most efficient grinding agents; and the three ingredients are rubbed or ground together for a long time, until they are converted into an impalpable powder of uniform tint. Samples of this powder have been examined for me by Mr. Brownen, F.C.S., whose skill in all pharmaceutical matters is well known, and he reports that the individual particles vary in magnitude from the 110th to the 600th of an inch in diameter. The smallest particles would be those of sulphate of potash, but let us take the mean, 350th of an inch, as the average size of the opium particles; 122,500 of such particles would cover a square inch, and I should conjecture that two layers of them of this extent would weigh a grain. If so, we have 245,000 particles of opium in the grain. We add to these 99 grains of sugar of milk, and triturate the prescribed number of times. Assuming, which is a very large assumption, that the admixture of the two powders is complete, a grain of the result would contain 2,450 particles of opium. This grain is added to a further 99 grains of sugar; and each grain of the mixture should then contain 24 particles and half a particle (how is the half particle to be obtained?) of opium. Such a grain added to 99 grains more of sugar will contain enough opium particles to supply 24 grains of the result with a particle each, while the other 76 grains will be left destitute. This will be the third so-called attenuation, which is supposed to leave in the material of each globule a millionth of a grain of the medicine; but which, if the medicine can only be divided into a quarter of a million of pieces, can only furnish one globule out of four with a dose of four times the presumed quantity.

In the case of fluids we have less definite guidance, and it is impossible to say to what extent a drop of spirit and water, holding some medicinal matter in suspension or solution, may be split up and distributed by admixture with a larger bulk. The apparently equal division of colour, which occurs when a drop of carmine solution or of blood is added to a pint or so of water, is of no value as a test, because the structure of the eye renders what, in relation to the subject before us, would be only a coarse mottling quite undistinguishable from uniformity. The colour of human blood, for example, is due to the suspension in fluid of red disc-like particles, which individually measure from the 3,000th to the 5,000th of an inch in diameter, and have an average thickness of the 10,000th of an inch. The most minute artificial division into particles of which we have any knowledge is that which exists in fatty emulsions, as naturally in milk, the white colour of which is due to suspended fat globules. Mr. Brownen tells me that the finest known division of fat in an emulsion gives particles ranging in diameter from the 20,000th to the 30,000th of an inch; but a particle of medicine of the 30,000th of an inch in diameter would far more than suffice, on homœopathic principles, to medicate the combined Atlantic and Pacific Oceans. In the absence of exact knowledge, we may, in the words of South, avail ourselves of the expedients by which reason supplies the want of the reports of sense; and it is obvious that the finest conceivable division of a drop of fluid must be into the molecules of which it is composed.

I speak from recollection, when I say that Sir William Thompson has lately endeavoured to give a notion of the probable size of the molecules of water, by saying that, if a drop of water were magnified to the size of the earth, the individual molecules would appear to be something between cricket balls and cannon balls. Let us suppose them to be spheres 3 in. in diameter. A cubic foot would contain 64 of such spheres, and the earth contains approximately 4,000,000,000 of cubic feet. Hence, on the scale supposed, the molecules in a drop of water would be 256 thousand millions in number, or 256 followed by nine 0's. That is, they would be only 6,000,000 more than a quarter of a billion, and their total number would fall short of a decillion, which is expressed by unity followed by 60 0's, by no less than 49 places of figures! A division into millionths of a drop is therefore conceivable; but a division into billionths, a favourite homœopathic quantity, would require the splitting up of each molecule into four parts—that is, its separation into its constituent atoms—with corresponding loss of its identity. It must be remembered, moreover, that the possible millionth is only predicable of the drop itself, and not at all of the medication of which it is the carrier or menstruum. I think these considerations show that the supposed homœopathic divisions are unattainable; and therefore render superfluous such calculations as those of Sir James Simpson, who showed that, if a man had to take a grain of medicine divided into billionths, each of which was contained in a globule, he would have to swallow one globule every second, night and day without ceasing, for 30,000 years in order to accomplish his task.

Having thus assigned reasons for believing the supposed attenuations to be impossible, I shall perhaps be pardoned for also assigning reasons for the belief that they are not even attempted. Several years ago, I was in conversation with a very scrupulous and conscientious man, a chemist in a provincial town, who kept a stock of homœopathic medicines for any customers who might wish to purchase them. He told me that he had recently engaged an assistant, a qualified chemist and a member of the Pharmaceutical Society, who had been employed, before coming to him, in the business of well-known homœopathic chemists in London. On the very morning of our conversation, a lady came into my informant's shop, and asked him for certain globules. He expressed regret that he had none left of this particular kind, but undertook to telegraph to London and to have a supply sent down by an early train. As soon as the lady left the shop, the new assistant expostulated with his employer, and assured him that where he had been there was no difference between the globules, but that they were sent from the manufacturers in bulk, and were used to refill any bottles which happened to be getting low. My informant was greatly shocked, and told me the story on account of the strong impression which it made upon his mind. In the *Medical Times* for 1858 there is an editorial paragraph to the effect that a patient, described as being, at the time of writing, a "respectable man," had told his doctor that he had formerly been a manufacturer of globules to a renowned London homœopath, who made enormous profits by retailing chests of these globules to his patients, at prices ranging from two to twelve guineas. In these chests the

globules were contained in many bottles differently labelled, and precise directions were given for their use. The sometime manufacturer positively asserted that no medicine of any kind was put into any of them; but that, however labelled, they consisted only of sugar of milk. He added that he gave up the business because he was ashamed of lending himself to it. In the same volume, in the number for the following week, a correspondent writes that he was consulted by a young man, who described himself as a confectioner. Being asked the nature of his work, the patient replied that he had been engaged for a considerable time in manufacturing globules for the homœopathic chemists in London. He said that these globules were made exclusively of sugar of milk, which was run through a sieve in a state of fusion, and that he had never known any medicinal ingredient to be added to them. He further said that no such addition was possible, for that, as soon as the globules left his hands, or as soon as they were dry, they were put into bottles for homœopathic medicine chests. He ultimately presented the writer of the letter with specimens of his handiwork, in about thirty bottles, labelled as aconite, mercurius, lobelia, ignatia, nux, pulsatilla, camomilla, and so forth. I cannot find that these statements were denied or questioned when they were published; and if we remember known facts with reference to adulteration, which, at least, on a large scale, can only be practised by people whose prosperity would seem to entitle them to be called "respectable," I do not think it incredible that globule manufacturers should be guilty of an omission which it would be absolutely impossible for their customers to detect. The "mother tinctures," as they are called, of the homœopaths are so strong as to be, in many instances, very dangerous poisons; but in the professed dilutions, whether tinctures or globules, the most refined methods of analysis fail to discover any trace of the matters which they are alleged to contain. The results of some examinations by Mr. Brownen on this point were read before the Medical Society of London in 1876 by Dr. Farquharson, M.P.

The public, sir, are, and must remain, absolutely unable to judge of the value of the technical arguments, whether in medical science or in any other; and the best thing they can do is to select their advisers with the greatest possible care, and then to trust them implicitly. But I can assure them that what is called the "law of similars" by persons who are, it seems to me, imperfectly acquainted with the meaning of common English words is at least as old as the time of Hippocrates, that it is well understood in the medical profession, and that it is regarded by competent judges as a grotesque exaggeration of a few facts which have, no doubt, their place in medicine, but which are only susceptible of very restricted application. It is on these few facts, however, and on the admitted readiness of some homœopaths to throw overboard, in the presence of any danger, the practice of infinitesimal dosing which Hahnemann regarded as essential, that the whole system of homœopathy, or what remains of it, is based; and the result affords an apt illustration of the truth of Lord Tennyson's lines:—

> "A lie which is all a lie may be met and fought with outright,
> But a lie which is part a truth is a harder matter to fight."

I venture to think that the so-called law "received a deadly blow in your leading article of the 4th inst., in which, replying to my objection that the symptom supposed to be caused by a drug has really nothing in common with the disease which the drug is used to cure, you say, "Perhaps not, but what does it matter?" Surely it matters thus far, that, if an unreal similarity leads the prescriber to a right selection of medicine, a real similarity would lead him to a wrong selection. From this dilemma I can perceive no way of escape; and if there be none, what becomes of the "law of similars"?

I am glad to see that no one has attempted to dispute my account of the intellectual barrenness of homœopaths; and this barrenness is as manifest within their profession as beyond it. Medical science finds ample scope for non-contentious work, in anatomy, in pathology (which some of your correspondents say they study), in physiology, in diagnosis. The homœopaths have had nearly a century of opportunity, during which these departments of learning have been assiduously cultivated and rapidly growing; but the homœopaths have done nothing for their advancement. Not one fact, still less one principle, have they contributed to the sum of our scientific progress. If any one of them would write a scientific book, containing a rational argument resting upon facts carefully observed and properly recorded, such a book would command the careful attention of the profession, who are anxious to learn, and who can have no other desire than to cure their patients as quickly and as completely as possible. But the so-called homœopathic literature, so far as I have seen it, consists of pseudo-scientific jargon, addressed to the credulity of the vulgar; and this circumstance, perhaps more than any other, accounts for the low estimation in which the authors are held by regular practitioners. Minds accustomed to the reasonings of Hunter and Brodie, of Baillie and Watson, cannot tolerate the pamphlets from which Lord Grimthorpe is content to derive what he fancies to be information.

With regard to the relative value of pathology and of symptoms as guides in the selection of remedies, I think some non-medical readers may understand, although Lord Grimthorpe cannot, that a method which rests upon the intimate nature of morbid action contains within itself the seeds of constant progress and improvement, as this intimate nature is constantly more and more disclosed by observation and experiment. A method which rests only on external appearances must be condemned, in the nature of things, to remain stationary, unless these appearances themselves undergo alteration.

Dr. Dudgeon attempts to correct me with regard to Hahnemann's knowledge of the itch insect. Hahnemann could scarcely be ignorant that the existence of such an insect had been surmised by Avicenna in the tenth century, by St. Hildegard of Bingen in the twelfth, by Ambrose Paré in the sixteenth, and by several writers—Bonomo, de Geer, and others, at later periods. But its existence was not made known, either by Hahnemann or anyone else, until 1834, when it was demonstrated by M. Renucci, in Alibert's *clinique* at the Hôpital St. Louis. Renucci showed its exact place of abode in the eruption, and how at any time it might be picked out with the point of a needle; but he, strange to say, had derived his knowledge from peasant women in Corsica. His demonstrations afforded one of the sights of the day

in Paris, but some time had still to elapse before the insect was universally admitted to be the invariable and only cause of the disease.

The story of Meissonier's dog is almost too funny, but the moral to be drawn from it could not be determined without more precise knowledge of the facts. "Vets.," even "great" ones, like other people, are liable to error, and dogs are subject to rheumatic affections of their muscles, from which they speedily recover under the influence of warmth and rest. But the case reminds me of another, in which a young lady suffering severely from toothache went with her aunt into a pleasant and sunny garden, while her mother prepared, and eventually sent after her, a homœopathic globule, which, doubtless, like that of Meissonier's pupil, was of "a rather high dilution." It was dissolved in a half tumbler of water, and the solution was to be taken by teaspoonfuls at short intervals. The patient presently returned to the house completely relieved of pain, and her mother exclaimed, "I was sure the medicine would cure you." "Yes, mamma," was the reply, "but I threw it over a gooseberry bush!"

Mr. Millican, whose courteous mention of me I am bound to acknowledge, objects to my reference to an advertisement. Surely the word can have no application to him, for his name was published first in a law report, then by Lord Grimthorpe, and was throughout of the essence of the question. If he thought fit to write at all, and on this point he was the only proper judge, he was bound to write over his signature. But he knows as well as I do that the most highly-esteemed members of the medical profession avoid rather than seek publicity. Their feelings on the subject may arise from prejudice; but, if so, it is a prejudice which originates in self-respect and has a tendency to cling to gentlemen.

R. B. C.

"J. C. B." (2).

[*Jan.* 10.

In analogical reasoning Lord Grimthorpe does not improve. Abandoning without a struggle the misleading comparison which he instituted between the conduct of doctors in declining to consult with homœopaths and a hypothetical line of conduct on the part of the Bar, he now suggests a parallel which is still more untenable, and, indeed, startling. Because a grain of mustard seed, which is less than all seeds, when it is sown, groweth up and becometh greater than all herbs, therefore a single brick can build a house. That is, in all fairness, the argument with which Lord Grimthorpe, an eminent lawyer, a self-satisfied logician, "shut up" a medical practitioner, who perhaps thought it well to avoid discussion with so eccentric a dialectician, and which he now gravely submits to the readers of *The Times*. Because an exceedingly small amount of vaccine lymph which is charged with living organisms capable of indefinite multiplication can "do the business" in protecting against small-pox, therefore a globule containing a decillionth of arsenicum, which can never be more than a decillionth, can "do the business" in curing intermittent fever. Vaccination is, according to Lord Grimthorpe, "a purely homœopathic procedure." The beneficent discovery of Jenner—as sound a practitioner as ever lived—published in 1798 is to be ascribed to a portentous impostor

who gave to the world a concoction of nonsense in 1810, or we are to believe that the action of bacteria which permeate the blood and tissues, in exhausting the soil in which they grow, or in producing a chemical substance, or poison (a contrary), inimical to a second invasion of the same organism, is an illustration of the great and universal law, *similia similibus curantur*. Lord Grimthorpe ought not to require to be told that the vaccine lymph with which he was vaccinated was an adequate dose of a very potent agent, and not in any sense a homœopathic attenuation; that it was loaded with germs which were fruitful and multiplied and replenished his system, and made him very uncomfortable while doing so; and that the vesicle which formed on his arm contained some drops of lymph rich in the same germs, which might by cultivation have leavened the dough of British babydom for generations to come, had it been deemed prudent to draw a lymph supply from so irritable a source. He wants to know the size of the micro-parasites in disease. Well, they are colossal when compared to the decillionth of a grain, and although small in contrast to a hen's egg, yet many of them admit of being measured. But it is not so much the size as the number and prolificness of these micro-organisms that is important, and there can be no question that they are present during many acute diseases in swarms and hordes which, could they be strained from the blood and tissues, would make a very appreciable bulk. They are, at all events, absolutely incomparable to the decillionth of arsenicum in Lord Grimthorpe's pet pilule, which is, as he swallows it,

> "Like the snow-falls on the river,
> One moment white, then melts for ever,"

and which cannot increase its dimensions or reproduce its kind.

Some time ago a powder, one of a number prescribed by a homœopath in London for a lady who died under his care, was forwarded by her friends, who were anxious to ascertain what she had been taking, to Dr. Stevenson, of Guy's Hospital. After a delicate analysis that eminent chemist reported, "I can find nothing but sugar of milk." And this it is that doctors are asked to investigate, the effects of nothing. There may have been in the powder a decillionth of something, but to the chemist, the physiologist, and the pharmacologist it was practically non-existent; and yet this freedom-loving peer, Lord Grimthorpe, will insist that doctors should waste their time in testing speculations as to the effects of this problematical decillionth, in support of which not an atom of trustworthy evidence can be adduced. I am surprised he does not propose that the Meteorological Department should undertake an exhaustive inquiry into the protective influence of cauls, the life-saving efficacy of which is a matter of ancient faith, is attested by many remarkable experiences, and is still widely credited by our seafaring population.

Confronted by grave imputations on the honesty of homœopathy as a system, and on the good faith of some of those who practise it—imputations which have not been lightly made and which can be substantiated—no better answer occurs to Lord Grimthorpe than an off-hand *tu quoque*. He does not deny that large quantities of homœopathic globules are sold containing no particle of the medicaments after which they are named, but he affirms as a rejoinder, on the

authority of a leading but anonymous druggist, that doctors are in the habit of prescribing secretly for their patients various Hahnemann medicines. I felt sure that assertion was a baseless calumny, and I have to-day made inquiries in connection with it of four firms of London druggists of the highest standing—viz., Savory and Moore, Bell & Co., Corbyn & Co., and Squire. They unanimously and unhesitatingly declare that Lord Grimthorpe has been grossly imposed on. They have known no instances in which doctors have ordered homœopathic drugs in homœopathic doses, but they have known many—and this fact is particularly commended to Lord Grimthorpe's notice (it was mentioned before, but he ignored it)—in which homœopaths have prescribed for their patients powerful drugs in the ordinary doses. The head of a leading drug firm in Liverpool (Mr. Abram), to whom Lord Grimthorpe's letter has been submitted, bears testimony to exactly the same effect. He scouts the idea that doctors meddle with helpless globules, and adds that he has not infrequently been applied to by homœopathic chemists for ordinary drugs in quantities which precluded the notion that they were going to be homœopathically employed. And not only do homœopaths give ordinary doses of ordinary drugs to their patients, but they take the same themselves when the occasion demands. I can say of my own knowledge that homœopaths when attacked by dangerous illness are not slow in asking an orthodox doctor's aid, and that they swallow with the utmost docility and without protest the anything but homœopathic pills and potions recommended to them.

If Lord Grimthorpe has not already misgivings as to the honesty of the homœopathic fraternity, he will, I am satisfied, be shortly awakened on the subject by the exposure of the falsity of the statements with which they are cramming him, and for which he is somewhat recklessly making himself responsible. Again leaning on a homœopathic pamphlet, he tells us that there are 11,000 homœopathic practitioners in America. Now it will take a little time to refute that bold announcement, more especially as "America" is a vague term; but, including in America everything from Cape Horn to Cape Columbia, I am certain it is a monstrous exaggeration. In the meantime I can furnish him with some facts which can be supported, and which will perhaps induce him to receive henceforth with more caution the mendacious tracts and instructions with which he is being so copiously supplied. The Scotch are a clear-headed and practical people. Well, a reliable authority informs me that there are 2,000 registered medical men in Scotland, and that one may count the homœopaths among these upon the fingers. The Irish, on the other hand, are an impulsive and imaginative people, but they have apparently no taste for the home rule of the pilule, for homœopathy has, I am assured, no hold on them. There are in England, Scotland, and Ireland, according to the Homœopathic Medical Directory, 278 homœopathic practitioners. And yet Lord Grimthorpe was put up to tell us that there are 10,000 in England alone.

Indignant with one of his critics, because he has quoted a 52-year-old dictum of the illustrious Andral, Lord Grimthorpe immediately adduces some 37-year-old statistics, by nobody in particular, to illustrate the success of homœopathic practice. But his statistics are

worthless, and do not, in the bald shape in which he brings them forward, admit of serious consideration. It would be the height of rashness to draw conclusions from the death rates, more than the third of a century ago, in hospitals of the sanitary state and management of which we know nothing, and under different kinds of treatment which are not detailed. The mortality in hospitals and many other institutions was at that time high when compared with the rates now prevalent, and the difference between the death rates in similar institutions was far greater than we are now accustomed to. But accepting the statistics as they stand, it does not follow that they redound in any way to the credit of homœopathy. Take pneumonia, for example. In that disease, says Lord Grimthorpe, the mortality under allopathic treatment was 25·5 per cent., and under homœopathic 5·7. But at the very same period, as has been shown by Professor Bennett, the mortality from pneumonia fell in the Royal Infirmary of Edinburgh from 33·3 to 2·5 per cent. in consequence of the substitution of rational for heroic treatment. The allopathic treatment of the disease in Edinburgh gave far more favourable results than the vaunted homœopathic treatment of it in Vienna.

We need not feel surprised, however, should homœopathic hospitals invariably give lower death rates than ordinary hospitals, for they are certain to be resorted to by a less serious class of cases. Homœopathy is the creed of those who suffer from minor maladies, and cultivate elegant varieties of invalidism. No doubt it meets a want among the idle and frivolous. Medical instincts are deep rooted, and range from the cat that eats grass up to lordly intellects like that of Bacon, who had such implicit faith in a pinch of nitre. "All men," says Carlyle, "are born hypochondriacs," and there are certainly few women who do not feel an interest in medication. To a number of hypochondriacal manifestations homœopathy ministers well enough, and in the hands of amateur doctresses its remedies are less hazardous than calomel and laudanum. But it is, for the most part, moderately healthy men and women who amuse themselves with airy nothings. Of course a drowning man will catch at a straw if there is nothing else handy, but he will prefer a life belt if there be one within reach, and it is a matter of daily occurrence that those who have toyed with homœopathy, when there was little or nothing the matter, turn to orthodox medicine when they find themselves in real jeopardy, and it may well be, therefore, that homœopaths seldom lose a patient by death.

After all, however, we are reminded in our discussion with homœopaths that the empirical test is the ultimate and conclusive one, and by that test orthodox medicine may be content to abide, for it is accepted and approved after trial by vast majorities in every civilized country. People will take the medicine that does them good. Yes, but they should be quite sure that it does do them good. There was a pathetic picture in *Punch* a little while ago of a fever-stricken *gamin* turning on his bed and with a grateful smile exclaiming to the physician who had just taken his temperature, "Thank ye, doctor, that's done me a power o' good." And most homœopathic remedies do good in this way. They are as inert as the thermometer, but they create the expectation of relief, and a feeling of relief sometimes follows.

But in the treatment of disease the immediate and the ultimate good are not necessarily identical, and instantaneous relief is not always compatible with permanent cure. Lord Grimthorpe is in such haste to be cured that he will not wait even a few minutes for examination or inquiry as to the nature or origin of his illness. "Stop my cough!" he shouts peremptorily. But to stop his cough might under certain circumstances be to imperil his life, and it is only by tracing that symptom back to its pathological cause that we can prescribe for him with the hope of permanent benefit. And here we see a beautiful instance of the law of compensation in homœopathy, how the principle of infinitesimals neutralises, as it were, the principle of *similia similibus curantur*. Dispensing as homœopaths do with anatomy, physiology, and pathology, and treating a list of morbid symptoms by a list of drugs, they might play havoc were it not that their drugs are harmless and even powerless. It is no exaggeration to say that homœopaths dispense with pathology, for Dr. Dyce Brown tells us that while they take a sort of speculative interest in it they refuse to be guided by it when it comes to treatment. Neither is it an exaggeration to say that their drugs are frequently powerless, for the same authority holds that a decillionth is an attenuation which they could not do without. They still treat symptoms and follow that blind empiricism which is the very thing deprecated in the passages quoted by Lord Grimthorpe from Sir Benjamin Brodie and other eminent members of the medical profession. Scientific medicine labours to extricate herself from empiricism. Homœopathy glories in sinking deeper into it. The old days when a drug might be added to the Pharmacopœia because So-and-so had found it useful in such-and-such cases are past for ever. Strict tests, varied observations, minute experiments are exacted before any new remedy is allowed to pass muster, and I should not be surprised if we found that there dropped out of our text-books in the future such disputable statements as that small doses of ipecacuanha are useful in vomiting, although the omission of that particular statement would be almost cruel, such a course of consolation has it been to our homœopathic wanderers.

Too much consideration has, I think, been conceded to homœopathy, and when the final judgment of medical science is passed on it, that will, in all likelihood, be that it has been an unmixed evil from first to last. It is not, I maintain, entitled to the credit sometimes bestowed on it for having contributed to the change which has taken place in the treatment of disease—a change from a heroic to a milder system. That change has been wrought by the spirit of modern rationalism, and it would have occurred all the same had Hahnemann never lived. The late Professor Hughes Bennett, of Edinburgh, did more by his vigorous scepticism and trenchant criticism to reform medical practice and teach the curative power of nature, the importance of diet and regimen, and the hurtfulness of the excessive administration of powerful drugs than all the homœopaths who have ever prescribed globules. And as he did this and gave life and brilliance to the Edinburgh school, there was in an adjoining class-room that Professor Henderson alluded to by Dr. Dudgeon in his letter to you as a pillar and ornament of homœopathy, who, smitten with that blight and barrenness which fall on all who walk in the shadow of Hahne-

mann, was content to go on delivering dreary lectures on stale pathology, suppressing those opinions which were, he said, his guides in private practice.

Dr. Dudgeon would have us believe that Hahnemann's "Organon" was the result of long study and research. How these homœopaths disagree among themselves! I met one of them in a hotel a few years ago who told me that this "Organon," which he carried about with him in ecclesiastical binding and treated with such reverence, was the offspring of direct inspiration. No unaided human mind, he said, could have attained to the height of its great argument, and it was certain that a revelation had been vouchsafed to its author. This gentleman further confided to me that the most recent advance in homœopathy was the discovery that lycopodium (the spores of the club-moss, a common coating for pills) is the cure for pneumonia. Lycopodium, he explained, causes flapping of the nostrils, flapping of the nostrils is the characteristic symptom in that disease, and small doses of lycopodium are, therefore, its certain cure. I told him that his reasoning was remarkable, and that I saw only three difficulties in connection with it, the first being that lycopodium will not cause flapping of the nostrils unless taken as snuff, the second that flapping of the nostrils is not the characteristic symptom in pneumonia, and the third that the man who trusted entirely to lycopodium in the treatment of pneumonia would, in my estimation, be little short of a criminal.

In concluding, I would say that the way in which the name of Dr. Lauder Brunton has been imported into this controversy seems to me slightly disingenuous. The references made to him are calculated to create the impression that he is half a convert to homœopathy, or, at least, regards it with some favour. By the error of a copyist the names of one or two homœopathic remedies found their way into the index of diseases in the first edition of his great and admirable work in pharmacology and therapeutics (they have been expunged from the second and third editions), and by virtue of his generous disposition he spoke of homœopaths with more forbearance and less acerbity than they were entitled to expect in such a book. But Dr. Lauder Brunton's attitude towards homœopaths is that of his profession. He refers to their system as quackery, speaks of the falsity of their claims, and of the utterly erroneous nature of Hahnemann's conclusions, and he has had the honour of being roundly abused in *The Homœopathic Review* of June last.

J. C. B.

DR. THUDICHUM (2).

[*Jan.* 10.

I cannot allow Lord Grimthorpe and Mr. Millican to misdirect, as they have done, the issue of Sir James Simpson's ipecacuanha story. I excluded entirely the discussion of the question of the therapeutical value of this root. The record was quoted to prove this, namely, what became of a scientific fact after it had been four times manipulated by representative homœopathic writers.

The above-named correspondents also direct what they, perhaps, conceive to be sarcasm at the ipecacuanha story, the one by alluding

to its age, the other by terming it "mediæval." It is evident that neither of them is aware that the original proposition underlying it comes from Samuel Hahnemann himself, and was published in 1796 in "*Hufeland's Journal of Practical Medicine*," Vol. II., p. 503. It is strictly limited to "chronic tendency to vomiting without matter." "In such cases," says Hahnemann, "one gives it" (namely, the ipecacuanha) "in very small doses to excite more frequent retching," and by this artificially excited symptom of more frequent retching, the original symptom—namely, retching or vomiting without matter—is said to be gradually cured. Again, I expressly exclude the discussion of the question of fact.

The nature of the materials upon which some of your homœopathist correspondents rely to put forth their case may be seen from the following:—One quotes a number of names of otherwise obscure professors of pathology, who were practitioners of homœopathy, and amongst them that of Rapp of Tübingen. Now Rapp was professor of clinical medicine and physician to the University Hospital at Tübingen more than thirty years ago. As I have myself heard him lecture in his clinic, I know that he both practised and taught homœopathy after the pattern of Hahnemann. The effect of this upon the medical faculty and school was such that, in order to save both from destruction, the Würtemberg Government found itself obliged to remove Dr. Rapp from his professorship and from Tübingen. This historical lesson should be closely studied by those who want us, by persuasion or compulsion, to admit homœopaths to our public institutions.

The allegation that homœopaths had not received fair treatment can only be made by one who is unacquainted with medical literature. The first forty volumes of *Hufeland's Journal* contain fourteen papers from the pen of Hahnemann, including one written at the request of Hufeland himself. Homœopaths were not expelled by the medical profession; but, as a contemporary says truly, they broke away from the profession, and became schismatic by their own act and choice. At first thousands of German physicians accepted Hahnemann's propositions regarding the prevention and cure of scarlet fever. When the treatment failed either to prevent or cure, they justly reproached him with having misled them. His answer, coming nine years after the first friendly challenge of Hufeland, and seven years after the epidemic which excited the greatest clamour, stands recorded in *Hufeland's Journal*, 1806, vol. xxiv., p. 139. He invented a new disease, which no physician had seen before or has seen since, called it "Purpurfriesel," or purple miliaria, and accused physicians at large of having mistaken this for scarlatina. In this forged miliaria he admitted his preventive and curative treatment for scarlatina to be quite powerless, and, what is still more significant, he never gave any advice either for its prevention or its cure.

In this paper "our master," as homœopaths are wont to call him, says, *l.c.*, p. 145:—"My heart is free from deception." "I burst publicly, I confess it, into an invective, the violence of which I have long since repented." And then comes the result of the repentance, which he calls "apology." "My colleagues have been prevented from doing me justice, not by a base heart, but by a confusion." Such an apology after a forgery shows an ingenuousness which cannot be easily ruined.

But this man had carried on, since 1795, an anonymous warfare against the medical profession, under the græculising name of Arkesilas, in letters directed to Ekdemus, notably in *The New German Mercury*, which became so violent that Wieland, its editor, refused to insert any more of these attacks. The "Lachesis" was then dispensed in other newspapers, but it lasted forty years before the medical profession finally resolved to keep its revilers at a distance, and full sixty before the last of the notable ones among them was, as above related, removed from the last clinical chair they ever held.

Those of your correspondents who would wait to see whether I and my colleagues at the Queen's Jubilee Hospital were the spokesmen of the medical profession have probably taken notice of the endorsement which these views and principles of action have received in the correspondence in your columns and the entire medical press. As one of these endorsements proceeds from a member of the Medical Council, it may be considered to be sufficiently representative of the medical profession.

Mr. Millican must be gratified to see his services to homœopathists so rapidly appreciated by them that his admirers have started a public subscription list, now in print before me, the proceeds of which are to be presented to him as an indemnity or testimonial.

J. L. W. THUDICHUM, M.D.

DR. J. H. CLARKE.

[*Jan.* 10.

When a writer who "absolutely disclaims the imputation of *odium medicum*" goes on to air loftily his scorn for a certain section of his medical brethren, saying that he regards them as if they were inhabitants of a different planet from that which he adorns, we are constrained to ask whether there is much to choose between *odium medicum* and *contemptio medica*; and when the same writer speaks of these same members of the profession—men who have studied in the same schools, passed through the same course of training, and taken the same degrees as himself—as being "practically" not members of the profession at all, we know exactly how much value to attach to his impartiality and to the opinion he may confide to an expectant world on the principles and practice of those "inferior persons."

To one who, like myself, has practised both allopathically and homœopathically, the letter of your correspondent "R. B. C." affords no little amusement. It has been said that a controversialist, to be successful, should know his opponent's side of the case better than his opponent himself. "R. B. C.'s" special qualification for discussing homœopathy seems to be that he knows neither his opponent's side nor his own; and it is most entertaining to see him, wrapped in the mantle of his towering scorn, deliver himself of an account of homœopathy evolved, apparently, "from his own moral (or immoral) consciousness."

It must be a comfort to all allopathists, whose greatest dread is to know what homœopathy really is, to read, on the high authority of "R. B. C." (whose initials must, he seems to think, be known and cherished by every professional allopathist), what homœopathy is,

"properly speaking." But his account is not likely to delude any unprejudiced person—as your admirably judicious leader shows, Sir, that it has not deluded yourself. "R. B. C.'s" "properly speaking" account of homœopathy is just as much like the true account as the distorted image in a much-curved mirror is like the face looking into it.

To set forth in the light of truth the facts distorted in the contemptuous mirror of "R. B. C.'s" mind would require a longer space than I could hope to be allowed. But I trust, Sir, you will let me advise your readers to go to those who know something about homœopathy and its founder if they want to ascertain the truth, and not to opponents of the mental cast of "R. B. C."

"R. B. C." states that Hahnemann's discovery "was not the fruit of study or research." It is hard to conceive that a writer to *The Times* can be ignorant of the absolute falseness of this statement. Hahnemann was for years engaged in study and research in reference to his method before he made his discovery known to the world. How he was led by his studies to hit upon the idea, how he worked at it until it became a practical guide to prescribing are matters of history, which all the contempt in the world cannot affect in the slightest degree.

Hahnemann found that cinchona bark not only cured ague but produced in himself and others, when taken in health, a train of symptoms hardly distinguishable from ague. This observation has since been abundantly confirmed by others. He next found that other drugs which were known to cure certain diseases also had the power of setting up in the healthy symptoms resembling these diseases, and that these instances were merely expressions of a general law. He perceived that while pathological theories were always changing, the symptoms of patients were a fixed quantity; and that, as affording indications of the patient's real state and of the medicine likely to help him, symptoms were infinitely better guides than the changeable theories of the pathologists. Hahnemann's rule merely states that in order to cure a case of illness the symptoms of the patient must be taken in their entirety and compared with the symptoms produced by drugs in the healthy, and that that drug must be given which has produced symptoms most like those of the patient. This is the only practical rule of any value for prescribing that has ever been discovered, and this alone entitles homœopathy to be called "regular" medicine. "R. B. C." talks in his exalted way about pathology as if it were possible to get at pathology except through symptoms, or as if it were possible to "get behind" symptoms without paying them proper regard.

Hahnemann used all kinds of doses, from the crude substances to the very high ones. He made his attenuations in a special way, a point always ignored by allopathists. The question of dose has no "rule," and experience is the best guide in this matter.

"R. B. C." would deny to homœopathists the possession of any merit whatever. He ignores Hahnemann's work as a chemist and a hygienist, the literary and scientific work of Henderson, Chapman, Rutherfurd Russell, Drysdale, and Dudgeon. Dudgeon's sphygmograph is now used in all the hospitals, and is acknowledged to be the best that has ever been invented, though those who use it are loth to give the credit of the invention to a homœopathist. The last thing *contemptio medica* would be guilty of is giving honour where honour is due.

34, *Harrington-road, S.W.* JOHN H. CLARKE, M.D.

MR F. ROSS.

[*Jan.* 10.

I read your leading article on medical orthodoxy with interest, and was glad to find that your attempt to state the controversy as between the two schools was fair and impartial. I read also the two letters of your correspondents, signed "R. B. C." and "J. C. B.," and was astonished to find that any medical men occupying eminent positions in the profession could be guilty of making such absurd statements about a medical system which numbers among its believers thousands of the most intelligent and educated of mankind, and among its practitioners many most able and trustworthy medical men. Not to trespass needlessly upon your valuable space, permit me as a homœopathic chemist to call your attention to one or two statements set forth in the letter signed "R. B. C." He says:—

"The late Mr. Heckstall Smith once published a striking calculation to show what the decillionth of a grain or drop would mean. He took the orbit of the planet Uranus, and found that if he had a cubic measure, each side of which was equal to the square of the diameter of this orbit, it would be necessary to have this measure filled eleven times and a half in order to obtain sufficient water to dilute a drop of laudanum or other liquid in such a degree that each drop of the mixture would contain a decillionth of the drug. In the same way, the bulk of sugar of milk required in order so to dilute one grain of 'nutmeg' that each grain of the mixture should contain a decillionth of a grain of the spice would be greater than the whole bulk of the universe of which the earth forms part. Apart from this *reductio ad absurdum*, it is tolerably manifest that the three shakes would not suffice to produce a uniform mixture of two fluids of different specific gravity, and that the drop of the first attenuation could scarcely fail to contain either more or less than one-hundredth part of the original medicine."

Striking, indeed, if true; but what is the fact? In order to make the 30th dilution, each drop of which contains the decillionth part of a drop, the quantity of dilute or rectified spirit necessary for this operation would be 3,000 drops; or, to put it simply, 12½ tablespoonfuls. Similarly, the bulk of sugar of milk required in order to produce a mixture that will contain a decillionth of a grain of nutmeg would be exactly 3,000 grains—somewhat different quantities to those contained in the astounding statements above referred to.

The next statement is as follows, and runs thus:—"As for the domestic practice of homœopathy, it will perhaps surprise Lord Grimthorpe to be told that, according to current report in the drug trade, the pilules sold to old ladies do not contain a particle of the various medicaments after which they are respectively named. They are made in bulk of pure sugar, and are distributed, when finished, into bottles bearing different labels."

Would, I ask, any homœopathic chemist, who has his living to get, and whose reputation depends upon the integrity and fidelity with which he prepares his medicines, be such a fool as to jeopardise both his reputation and character by such a course as this? Besides, both the patient and medical man would soon detect the fraud, and instead

of being a gainer he would be a loser by his nefarious conduct. As a homœopathic chemist, I can bear testimony to the care, both in the selection of drugs and the strict regard paid in carrying out the pharmaceutical processes necessary to the preparation of homœopathic medicines exercised by all my brethren in the trade; and I feel sure that there is not one among them who would be guilty of such imposition, either upon their patrons in general or the medical men practising as homœopathic physicians. As to the law of cure which governs all true homœopaths—viz., that a medicine in order to be curative must be a true *similimum*, expressed by the formula *similia similibus curentur*—this has been tested in millions of instances during the last 80 years, and is established beyond all doubt as a therapeutic law. This does not by any means imply that diseases may not be cured by any other method. There are doubtless other means by which diseases may be successfully combated; but the question is, Which is the best? And experiment alone can decide this point satisfactorily. Most, or nearly all, homœopaths have tried the older, or so-called orthodox plan, and have found, by comparing the one system with the other, that the homœopathic, taking it all round, was by far the safest and quickest system for the cure of disease, and consequently they embraced it.

Then as to dosage. One dose is as good as another if it only cures, and it matters not, in my judgment, whether it be ten drops of the pure tincture or a millionth part of a drop of the same; but if the smaller does is effectual, common sense would say give me the smaller one. To those who have not studied the action of medicines prepared homœopathically, and seen the splendid results which follow the administration of a wisely selected drug, it must always appear absurd to expect a dangerous disease to yield to what seems such inadequate means. And it is only by observing the action of minute or infinitesimal doses upon sick people that the mind can realise how potent they are. But to ridicule a system and condemn all those who practice it as either knaves or fools, with the crude notions and erroneous conceptions which your correspondent "R. B. C." evidently has in regard to homœopathy, is neither courteous nor just. I go into my garden and stand upon the damp ground for a while, have a sudden attack of lumbago, I take aconite and rhus tox., each dose containing only the two-millionths of a drop, and in a few hours I am well. It may not be orthodox; may not square with "R. B. C.'s" notions of what ought to be; it is sufficient for me that I am relieved.

Such clear and unmistakable instances of the curative action of homœopathically prepared medicines might be adduced by hundreds; but why multiply them? There is not an intelligent father or mother in the three kingdoms who has used homœopathic medicines but can bear similar testimony to the action and value of simple remedies selected and administered on the homœopathic principle. Hundreds of artisans and working men can also bear witness to the value of aconite in fevers and feverish colds; of pulsatilla or nux v. in indigestion and liver derangements, even in doses not larger than the millionth of a grain (*plus* the development of latent power developed by the processes of succussion and trituration), and these will never, by any ridicule which "R. B. C." or "J. C. B." may bring to bear upon the homœopathic system, have their faith shaken in it, or their

confidence so far disturbed as to induce them ever to return to the so-called orthodox system. We who have tried homœopathic medicines for years and seen their operation feel that we have found "the more excellent way," and can well afford to let those who differ from us laugh and sneer while we go on our way rejoicing.

Apologising for this long letter, believe me,

Yours faithfully,

9, *Vere-street, Cavendish-square.* FREDERICK ROSS.

THE FACTS OF "R. B. C."

[*Jan.* 10.

Like your correspondent "Layman," whose letter appears in *The Times* of to-day, I can check one fact only in "R. B. C.'s" letter, and that is also incorrect.

"R. B. C." claims Dalton as one of the medical profession who "enriched mankind with notable discoveries." Surely every schoolboy knows that Dalton was a schoolmaster and "professional chemist." In "Pigot's Directory of Manchester and Salford" I find his name. "John Dalton, mathematical teacher, 10, George-street." For six years he was a tutor at the Manchester New College, and afterwards gave private lessons at 1s. 6d. to 2s. 6d. each in mathematics, &c., and made analyses at 10s. 6d. each.

Dalton was certainly a "Doctor," but it was Civil Law of Oxford, a distinction conferred upon him at the same time as Brewster, Faraday, and Robert Brown.

It is to be hoped that "R. B. C.'s" medical facts are more reliable than his biographical ones.

J. C

LORD GRIMTHORPE (5).

[*Jan.* 12.

These allopathic doctors have all but converted me to a belief, not only that homœopathy is as good as A, which I knew before, but a great deal better, and (if that is worth anything) that it is the only mode of medicine which has any pretence to be called a system or a science; for the A's proclaim among themselves, though not to us that their own has not. One need not know more of medicine than they do of reasoning to be quite sure that a set of controversalists who persist in shirking the real issues and charges against them, and spend a heap of superfluous energy in playing with the fringes, know that they have no real defence. They keep saying in every place and every letter, that they will not discuss any evidence of homœopathy, or any statistics, except an old assertion or two of their own, unsupported by a scrap of evidence; that *The Times* is not the place for it, nor its readers competent judges, nor the governors of any hospital, nor nobody, in short, except themselves. If I had never read, seen, heard, or tried a single homœopathic experiment I should know that such arguers as those are wrong, and conscious of their case being rotten.

But, in fact, they do discuss just as much of it as they think they can make anything of, and just as much as is not worth a farthing by

itself. If it were a proved result of experience that infinitely (in the popular sense) small doses of medicine do nothing, it might then be worth while to inquire why infinitely small doses of other things sometimes produce very serious effects. We do happen to know how and why vaccine virus acts. Saying that the stings of certain flies act because the flies are alive is explaining nothing; it is only asserting a bare and obvious fact. Do the allopathists know how poisoned arrows act, or how much poison gets into their finger and kills them sometimes by a scratch from a dissecting tool; or why a single whiff of a stink sometimes poisons people, while at other times the same or other people safely live in them for hours; or how much emanation of anything from a man enables a bloodhound to pursue and find him? Darwin wrote that he did not expect people would believe the infinitely small quantity of some substances, and some of them dead ones, which, put on the leaves of Drosera, make them perform curious antics. All those instances prove nothing positively for homœopathy, but do refute the *a priori* guess that infinitely small quantities of any medicine cannot produce the effects which every fair experimenter has found that they do. Yet that guess is the whole argument of these A's, and is obviously worthless against the facts which they dare not try to refute, and against the above cases of other kinds of small doses. For the purpose of any practical conclusion, all their elaborate calculations and displays of learning about how small particles can exist or can colour fluids and the like, are mere fireworks. They might as well say that gravity cannot act because nobody knows how it does or can act, either through dense matter or an apparent vacuum and instantaneously through all known distances.

And, speaking of gravity, I think the physicist of *The Saturday Review* has "whopped all creation" for folly in this line, and for impudent assumption of what he has to prove. For he says that the architects have no more found it necessary to pass a formal resolution in favour of gravity than any authorised medical council has against homœopathy, as if the truth of one and the falsehood of the other were equally indisputable. But such is the way of them. No surgical operation can drive it into their heads—either the Scotch or the English allopathic heads—that their infallibility is the very thing in question, and that calling their opponents liars and imposters, and H "a blind empiricism," while they refuse to let experiments be tried which would settle it in a year in any large hospital, proves that they know or fear that the experiments would settle them too. The same article repeats once more the now notorious falsehood that the A's only refuse to meet the H's in consultation, which the H's do not want any more than they do.

Here is another of their doings, sent to me since my last letter, by a person whose name would command attention if he allowed me to give it, or would send it you himself, which perhaps he dares not, for fear of more persecution. A "regular" told him that his daughter would die in a month from cancer. He ventured to consult a H, who told him she had nothing of the kind, though she was seriously ill, and, in fact, she died in a year of something else, as was then made clear. He asked the regular to examine her again, and he refused, because her father had touched the accursed thing and convicted

himself of heresy. It does not signify the least that she was incurable—if she was, which is by no means clear. What does signify is that one of these pretenders that they will only not consult with H's, deliberately allowed his patient to die (as far as he was concerned) because her father had tried to save her life by consulting a man who turned out right. Now we shall see whether "R. B. C." "J. C. B.," or Dr. Thudichum condemns such conduct as only the exceptional disgrace of an individual, and, for what he cared, manslaughter, or defend it as a piece of loyalty to their great principles. Thus far they have not attempted one word of defence of their persecutions, which we now see are not even confined to their own "wandering brethren," as "J. C. B." calls them. It is plain that they would rather patients died than be cured by a homœopathist.

[I learnt afterwards that this sad case was even worse than the father had told me at first. For not only one "regular" but three, of different orders, all concurred in assuring him positively that she had cancer in one kidney, and that after cutting into her to see and holding what they call a consultation. And yet after the *post mortem*, which was not by a H, or they would summarily have rejected it, had put it beyond doubt that she had not, but a perhaps curable disease if they had found it out, they began disputing whether one of them had not told the others that they were wrong. I was shown all their letters, and it is quite plain that one of them did. The third, a general practitioner, unfortunately suffered a complete paralysis of memory as to who said what at the consultation, which is not difficult to account for in the circumstances. So observe, first, the tremendous humbug of a medical consultation; secondly, that when the father was told they all agreed in pronouncing for cancer, he was told what (in Jubilistic and allopathic language against every H) was a lie; thirdly, that an ignorant old H doctor divined better what was the matter with the poor lady, from a symptom which they had never even asked about, than those three A's did, or said they did, by cutting into her and actually seeing the organ before them; and yet, fourthly, because he had done so, they refused even to reconsider their verdict (for I suppose they all concurred in that too, or would have done). This is orthodox "medical ethics," which except a man believes faithfully, he is an impostor and a quack, who must be excommunicated and ruined if possible, and so must those who "meet" or employ him.]

But there is a far more striking omission in the letters of all these three doctors. Just imagine three champions of any other profession filling three columns of *The Times* again with all sorts of arguments, whether good or bad, and all, by a unanimous instinct of self-preservation, saying not a word in answer to that tremendous, and I really thought, hardly credible, exposure of the sayings of not one or two disappointed or ill-tempered men of their own, but of more than 30, of all kinds and degrees, including some of the past and present leaders of their profession. Their vilification of homœopathists and homœopathy in any specific way, beyond calling them liars, is feeble compared with their particular and precise descriptions of the state of their own arts and methods of curing or deceiving patients. I need not quote them again; but anyone who read the few I did quote

before, or will look at them, will see that they amount in substance to this sort of thing—Gentlemen, "here we are again." We have to say something, you know, about medical progress and science. But, between you and me, there is no such thing. "I deny that we have a scientific use of medicines"—(Dr. Wilks). "We have to make our patients believe, when their minds are shaken by sickness and anxiety, that we can cure them" (Moxon), but we know that is all nonsense. If they are curable at all, nature will cure them, if we do not manage to prevent it—(G. Johnson and others). "It is my conscientious opinion, founded on long observation and reflection, that there would be less sickness and mortality if there were not a single physician, apothecary, &c., in the world"—(J. Johnson, editor of the *Medico-Chirurgical Review*). They have had plenty of time to read the tract I quoted and to try if they could convict it of forgery. They know better.

In full accordance with all this I remember one of the most distinguished living philosophers (not medical) lauding a certain then highly fashionable physician one day at dinner. I had reason for asking him, and I did ask, Did you ever know him do any good to anybody seriously ill? He answered, "Well, if you put it in that way (a way quite immaterial, of course, to his value), I cannot say that I ever did." As "R. B. C." and Co. treat us to a variety of reports, here is another little one. A general practitioner, and commonly reputed a very good one, said to a relation of mine one day, I have been sending out a box of pills which I should be very sorry to take; and another calmly advised a friend never to take any of their medicines. So it is not only homœopathic medicines that are, or ought to be, "thrown into the gooseberry bush," if these candid administrators are right about their own doses and doings. They might very well finish any of those domestic orations to their fellows thus—"So you see, gentlemen, the dramatists and satirists of all ages have been right in representing us as humbugs. But mind you, we must have no homœopathy, no hydropathy, no bone-setters. We must talk to *The Times* and the public about our guiding lights of medicine, great principles, ours being the only real science of medicine, and so forth. If experience is thrown in our teeth, we have only to say that all such testimony can easily be beaten by the 'puffs' of Holloway's ointment, Dr. Jacob's oil, and the like;" which is just what one of them wrote to me the other day, a man of ability and science in other ways, and, for anything I know, as successful as any of his brethren in letting nature cure his patients when she will. Even those "patent medicines," which succeed in making the fortunes of their inventors, must be included in the "remedies of empirical origin which have stood the test of time and experience," as was admitted by Sir R. Christison, who caused the clauses for protecting H to be introduced into the Act by trying to extort pledges not to practise it, and yet said his own art was in a backward and unsatisfactory condition.

They refuse to discuss the published statistics of homœopathy, being from twice to four times as successful as A in every hospital where it has been fairly tried, though they knew they were official and done under the eyes of A's themselves, and could not be "cooked." And not a word has "R. B. C." or any one of them to say about their disgraceful attempt to suppress the cholera returns from the London

Homœopathic Hospital in 1854, till the House of Commons made them disgorge it. I thought when I quoted that from a homœopathic tract that they would surely try to dispose of it somehow; but again they are silent, as they are about all the worst things against them. It is a small specimen as to quantity, but was it a "puff" in "Trollope's Recollections" that his mother was cured in three days, and enabled to go to Court, of erysipelas which made her head double its proper size, by a pupil of Hahnemann's, whom Princess Metternich sent to her? or was the well-known cure of Marshal Radetsky of a dangerous tumour by Hydrastis, a H medicine, a "puff?" If it was, it was a very lucky one for a lady in my family, who read it in a newspaper, after having an eye demolished in effect by a great London surgeon for a tumour, which soon began to grow again, till she stopped it with that medicine, and lived thirty-two years after in perfectly good health. I have now a letter from a gentleman saying that two most eminent doctors had condemned a lady in his family to a speedy death from cancer; so in despair he went to a H doctor, and was well abused for it, but the disease was stopped, and this lady "is now wonderfully well for a person of seventy-eight." These A's have not the sense to perceive the difference between advertised puffs of medicines by the sellers of them, and such stories as these by persons who have no interest in lying. Can they say as much? All the experience they are giving us leads me to believe that the H stories are infinitely more true than theirs. If the old experiments were not to be relied on, why do they make everybody they can refuse to let new ones be tried? That refusal alone is absolutely fatal to their case and to their credit, even if they had not told us not to accept it, and to regard them as what they confess themselves among themselves.

I am struck with one or two little incidental symptoms of their honesty in another direction. Why does "J. C. B." repeat again my twice-corrected clerical error, which was also transparent on the face of it, of 10,000 H practitioners in "England" for "the world?" He may have missed one correction, but could not possibly have missed the second. It wss one of those curious slips which Mr. Proctor well described from his own experience in *Knowledge*, a few years ago. Whenever I see anything that looks like one in a controversy, I suggest that it may be so; sometimes I find that I have been too charitable. This allopathist, having nothing to say in defence of his own side, condescends to such rubbish as dilating on an obvious and twice-corrected clerical error. But he has a much bigger one to explain away for himself or his party. The homœopathists naturally availed themselves, at Margaret-street and elsewhere, of the insertion of not "one or two," but a great many "homœopathic remedies" in the A *Materia Medica*, or some such book, by an avowed opponent of homœopathy, but an equal denouncer of the fallacy and vagueness of his own art, Dr. Lauder Brunton. The contrast between the anti-homœopathic demonstrations by him and another pharmacopœist, Dr. Ringer, and their esoteric recommendations of H medicines to their own trade, is delightfully exhibited in Mr. Millican's article in *The Nineteenth Century* of this month (Feb., 1888). The Jubilee persecutors must wish by this time they had been quiet.

Now we are told they all got in by the "error of a copyist" (he must mean all those errors), and we are expected to believe it. Was it by the error of a copyist that the *Lancet*, two years ago, published that entirely false account of Dr. Moxon's death? Here is another funny little indication:—"'J. C. B.' would not be surprised if we find dropped out of our text-books for the future such disputable statements as that small doses of ipecacuanha are useful in vomiting, although (he means, because) that particular statement is a source of consolation to our homœopathic wanderers." Nor should I; nor at any other anti-homœopathic amendment of the A textbooks. They want purging, clearly. "J. C. B." may write his fingers off before he will persuade mankind that the great diminution of allopathic doses is not due to Hahnemann and the H's, but to some unknown Scotch benefactor named Bennett, or to "modern rationalism," whatever that means; probably it means something as definite as the "empiricism from which scientific medicine labours to extricate itself," according to him; while according to Professor Stokes of Dublin (orthodox) "medicine is now simply empiricism," and according to Dr. Wilks, of Guy's, "it has no scientific basis, but is formed out of the fancies of the human mind."

What has come to the *Lancet*, I wonder. Is it because Hutton, who used to fill this street (Queen Anne) with cabs, is dead, or because Sir James Paget is too great a man for that unscrupulous phlebotomist to revile, that the *Lancet* thinks it best to say in answer to me that he "has done magnanimous justice to the bone-setters," whose chloroformist the regulars boycotted out of serving Hutton any more? "Justice," is it? Then what have all of them, except Sir James, been doing all this time, assuring us that Hutton knew nothing and cured nobody, but spoilt many people or their limbs for ever? He cured five people in my family alone after the regulars had failed utterly, and I do not know how many others that I knew. But homœopathy is enough to discuss just now, and I see a successor to Hutton has set up his tent, or his brass plate, next door to him; so the *Lancet* had better not be too candid in a hurry. Mr. Ross has dealt sufficiently with one of the mare's nest stories of these people; and, as it happens, I can personally confirm what he says about lumbago, having been cured last year of the worst fit of it I ever had, and in the least time, by a homœopathic doctor. There is nothing worth notice in Dr. Thudichum's letter or their other stories or contradictions. Nor do I see any use in exposing people any further who expose themselves as these writers have done equally by their silence and their speech. They have lighted a jubilee candle which will not soon go out. The time will come when another Hume will write of them as he did of their fathers' persecution of Harvey. He says that not a single contemporaneous physician above forty years of age ever accepted the circulation of the blood.

GRIMTHORPE.

DR. MILLICAN (4).

[*Jan.* 12.

I am thankful that my share in this controversy is now practically done. I have adduced in all sincerity certain reasons for thinking that the position of exclusion adopted by orthodox practitioners

towards homœopathists has no longer any firm basis, and I fail to have observed one single argument—with all due respect to my opponents—that in the slightest degree shakes the platform I took. I have shown that the profession, as represented by such authorities as Brunton, Austin Flint, *The Lancet*, and others, has discarded the scientific in favour of the ethical objection to homœopaths—viz., that they are separatists and disturbers of the unity of the profession. But when a large section of the profession is so bigoted that (as you admirably express it in your leader) it "will not tolerate a man who tolerates a heretic," how can the homœopaths be expected to put credence in the appeals which are made to them to abandon their works of self-defence and accept the terms of truce held out to them?

The bulk of the scientific arguments are directed against the system of Hahnemann as a whole. To this system no homœopath is pledged merely by reason of his being one. Therefore, whatever insurmountable differences there may be between the profession at large and individual practitioners of homœopathy, they cannot be predicated of the lot, or even of the large majority.

One word in reference to the infinitesimal theory. This is not of the essence of homœopathy, even if it be of Hahnemannism, but a detail of experience to be decided by each practitioner for himself. Therefore, even if it should be shown to be full of absurdities, or beyond the pale of reason in theory, its existence as part of Hahnemann's scheme is no valid argument in favour of the refusal of professional intercourse to homœopaths.

But I have two considerations bearing upon this subject to submit:—

Michael Foster says: "Odorous particles present in the inspired air . . . falling on the olfactory epithelium produce sensory impulses which, ascending to the brain, give rise to sensations of smell." ("Text-book of Physiology," 3rd edition, 1879, p. 522.)

A grain of musk is exposed in a large room. Say the room is 15ft. in each dimension, then its cubic capacity will be 3,375 cubic feet. The air in this room will be entirely changed several times per diem by ordinary diffusion with such natural ventilators as the chimneys, chinks in the window and door fittings, and crevices in the walls and floors. Yet the smell of musk—that is to say, the presence in the atmosphere of particles of musk capable of producing a nerve reaction in the human organism—will persist in the room for days. This is an experiment which anyone can try for himself.

Secondly, are the material particles given off by a man in rapid flight across country "thinkable" divisions of matter, or do they bear a thinkable relation to their surroundings? Yet they are sufficient to to produce a material contact with the olfactory nerves of the blood-hound who tracks him. Is it, therefore, so very "unthinkable" that infinitely more minute drug atoms than we are accustomed to credit with any therapeutic powers may, when in a state of intense subdivision and solubility, which may be compared to the diffusion of the odorous particles in a gaseous medium to which Foster refers (*eod. loco*, p. 523) as "necessary for the development of smell," induce a physiological reaction of the organism?

One more instance. I have before me an original glass receptacle

which came from Constantinople many years ago with attar of roses. The inside of that receptacle has been absolutely free from perceptible contents of any kind for upwards of twenty years. Yet there are material rose particles therein at this moment, enough to scent even the box in which it lies.

I submit these facts as supplementary to the remarks in my letter of the 20th ultimo, in proof of the contention that even if infinitesimals be included, the leading and essential principles of homœopathy are not axiomatically absurd. Whether they are true or not is a matter of evidence, and evidence of a nature the negative side of which is always liable to change. I have not seen them proved in a sufficient number of cases to convince me of their general application, but I cannot, therefore, impugn either the honesty or the intelligence of those who say they have.

In the matter of professional advertisement referred to by "R. B. C.," whose disclaimer I frankly accept, I would say this. To parade in public one's professional achievements, abilities, or reputation may be objectionable, though even this is permitted in the case of bulletins relating to persons of high rank whose condition is a source of genera interest; but to brand the acceptance of publicity in questions of an impersonal nature such as this, and whose bearing upon the public liberty and welfare is indisputable, and to regard any form of publicity as due to a prejudice "which originates in self-respect and which has a tendency to cling to gentlemen," would be to inhibit all kinds of work in the public interest on account of the inevitable prominence they entail, and to overwhelm in common reproach political leaders, social reformers, bishops, scientists, and even lecturers on sanitary or other topics to the Kensington exhibitions or the admirable ambulance classes, whose establishment has proved a boon to the community at large.

The only point that calls for notice in Dr. Thudichum's letter is the assertion that I misdirected the issue of Sir James Simpson's ipecacuanha story. If the issue was not such as I stated it, then it was entirely irrelevant. I might add that the contemptuous tone of his final paragraph, wherein he gives me news which, I fear, in view of possible law expenses, my modicum of human nature will not allow me to regard altogether with unmitigated dissatisfaction, rings somewhat with the hollow clang of envy.

KENNETH MILLICAN.

DR. R. E. DUDGEON (2).

[*Jan.* 12.

The Times of this morning contains an allopathic dose of homœopathy. I wish I could make my reply more in conformity with our posology, but I fear that is hardly possible. "R. B. C.," nothing daunted by the slight effect produced by the astronomical calculations of Smith, now tries the chemical calculations of Brown—I beg pardon, Brownen—and the molecular calculations of Thomson. These elaborate sums in arithmetic have no bearing on the subject, but if you like to give "R. B. C." space to display them we cannot object; they amuse him and they do not hurt us. After all, the question is, Does homœopathy cure more, more quickly, and more pleasantly than the old system?

Lord Grimthorpe has given you some statistics from our opponent Dr. Routh and others which show that it cures more; the experience of Tessier, in the Hôpital Ste. Marguerite of Paris, and many others shows that it cures more quickly; and everyone knows that it cures more pleasantly. "R. B. C.'s" calculations remind me of the mathematician who had proved to his own satisfaction that there was no such thing as motion, but failed to convince his friend, who practically proved that there was by walking across the room—*solvitur ambulando.* No doubt the contemporaries of Galileo proved mathematically as well as theologically that the earth did not move, but the correctness of Galileo's "E pur si muove" has been sufficiently proved.

"R. B. C." has apparently been inquiring into the history of the itch insect, since I told him Hahnemann knew all about it; but his statement then was, "In those days it was not known that real itch is caused by a parasitic insect which burrows in the skin," and he even now says that its existence was not made known either by Hahnemann or anyone else until 1834. Now, in a paper entitled *Der Anzeiger—Anglicè, The Advertiser*—on the 31st of July, 1792, after corroborating the statement of another contributor to that periodical that the cause of itch was certain small insects that burrow beneath the epidermis, for which he quotes the authority of August Hauptmann, Bonome, and Schwiebe, Hahnemann says:—"These exceedingly small animals are a kind of mite; Wichmann has given a drawing of them; Dover, Legazi, and others have observed them." So much for "R. B. C.'s" newly acquired knowledge of the subject.

"R. B. C." is "glad to see that no one has attempted to dispute my account of the intellectual barrenness of homœopaths." Well, we are a modest as well as a feeble folk, and we do not care to lay claim to all the learning and science, let alone the moral virtues of honesty and truthfulness, which our opponents are never tired of claiming for themselves. But when challenged in this way by "R. B. C.," we may be allowed to give a few testimonials in our favour from our opponents. And first of Hahnemann. Hufeland, called the Nestor of Medicine by his German contemporaries, who knew Hahnemann intimately, says:—"Having been connected with him for more than thirty years, by ties both of friendship and of letters, I have always esteemed him as one of our most distinguished, intelligent, and original physicians." I could give many similar testimonials to Hahnemann's learning and genius from eminent medical men, his countrymen, but I shall only quote one other from our own countryman, Sir John Forbes, a courteous though earnest opponent of homœopathy:—"Hahnemann was undoubtedly a man of genius and a scholar, a man of indefatigable industry, of undaunted energy. In the history of medicine his name will appear in the same list with those of the greatest systematists and theorists, surpassed by few in the originality and ingenuity of his views, superior to most in having substantiated and carried out his doctrines into actual and extensive practice." Now for his disciples, and here my modesty will only allow me to quote one allopathic authority, Dr. Bristowe, who, in his address at the meeting of the British Medical Association in 1881, said, speaking of practitioners of homœopathy:—"It is absolutely certain that men of learning and ability are contained within their ranks. If you care to dive into

homœopathic literature, you will find in it plenty of literary ability, and I have perused many papers by homœopaths on philosophical and other subjects unconnected with homœopathy which prove their authors to be men of thought and culture, and from which I have derived pleasure and profit."

That the department of pathology which "R. B. C." says we are specially deficient in owes something to a homœopath may be seen from the notice of Professor Henderson, of Edinburgh, in *The Lancet*, at the time of his death. This allopathic periodical says Henderson was the first to "notice the murmur of efflux in a case of sacculated aortic aneurism; while he was also the first to demonstrate as a diagnostic sign of aortic regurgitation that the radial pulse followed that of the heart by a longer interval than usual." "He was the first to show on irrefutable grounds that these two forms (typhus and relapsing fever) were in reality distinct and were due to different causes." I may add that he was the first to show the cause of the sudden subsidence of the febrile symptoms in pneumonia."

"R. B. C." still harps upon Andral's pretended trial of homœopathy, though it is hard to see how a trial of homœopathy could be made by a man who knew nothing about it, who had previously asserted that it was a delusion, and set about to prove it so. Such ignorant trials are occasionally made even nowadays. In the 13th volume of *The Practitioner*, p. 241, Mr. R. Brudenell Carter (same initials, by the bye, as "R. B. C.," of course a mere coincidence) gives an account of his treatment of the late Dr. Anstie in his last illness. Dr. A. had pricked his finger while dissecting, and four days afterwards Mr. Carter saw him, and "found him in bed with a perfectly dry, hot skin, a tongue so dry that he could scarcely articulate, and complaining of intense headache." Now if there is one homœopathic bit of treatment better known than another to all the world it is the power of aconite to allay certain kinds of acute fever. So Mr. Carter, thinking no doubt this was a case for aconite, prescribed "a minim" (*Anglicè* one drop) "of the tincture in water every hour until diaphoresis" (*Anglicè*, sweating) "was produced." But, as everyone acquainted with homœopathy could have told him, that is not the sort of fever for which aconite is useful—so it did no good, perhaps harm. This was the sort of homœopathy Andral tried, and with the same result as Mr. Brudenell Carter. "A little (homœopathic) learning is a dangerous thing."

Possibly "R. B. C." may think that the public will believe his anonymous stories of the dishonesty of homœopathic chemists. Well, I will not attempt to set bounds to human credulity, but I think most sensible people will consider them as belonging to the "cock and bull" kind.

I am sorry to trespass on your space so much, but I must say that "J. C. B.," in his persistent accusations of dishonesty against his colleagues, reminds me of a motion proposed at an extraordinary meeting of the Fellows of the Royal College of Physicians on December 27, 1881, by Dr. J. C. Bucknill (same initials as your correspondent—again a mere coincidence), "That no competent medical man can honestly practise the so-called homœopathic system." This was too strong even for the Royal College of Physicians, so it was negatived. Perhaps your readers will think "J. C. B.'s" accusations of dishonesty

against colleagues as well educated and probably just as honest as himself a trifle too strong.

Dr. Thudichum's reference to Hahnemann's essay of 1796 seems to me to be *à propos* of nothing. At that time Hahnemann had done nothing more than suggest that remedies which caused symptoms similar to the disease might perhaps be useful in some chronic affections, but the homœopathic system of treatment was not excogitated by him till long after that.

Rapp was professor of clinical medicine and special pathology at Tübingen from 1850 to 1854. I am quite aware that it was alleged that his presence in the University was the cause of a great falling off in the number of students. But this, like many other of our opponents' statements, was incorrect. It was shown from the inscription list of the University that while in his six summer and winter courses he had 289 students for his class of clinical medicine and 149 for his class of special pathology, his predecessor, Wunderlich, during a similar period, had only 191 and 99 students in these classes, and his successor, Griesinger, during a similar period, 254 and 124 students respectively. It was the *odium medicum* of his fellows in the faculty which brought about his removal, not his want of success as a teacher. The Government knew this very well, and gave him a post unconnected with the University of equal pecuniary value.

R. E. DUDGEON, M.D.

DR. D. DYCE BROWN (2).

[*Jan.* 12.

The general tone of "R. B. C.'s" letter is such that my first impulse was to consider it unworthy of reply. But as certain statements are made in it which might be expected not to remain unchallenged, will you permit me a few words? He tells us that Lord Grimthorpe "absolutely compares vaccination to homœopathic medication." Lord Grimthorpe is quite correct. The use of vaccine lymph as a preventive of smallpox is that of a substance which produces in the healthy human body a disorder which is very similar in most points to smallpox. The appearance of the vesicle, the concomitant fever, the period of maturation, and the gradual falling off of the scab, leaving a permanent scar, are points which mark out strongly the similarity of the one disease to the other. And that the similarity is not a superficial one is shown by the fact that if a cow is inoculated with smallpox matter the result is vaccinia. By being passed through the body of the cow, an animal not susceptible to smallpox, the smallpox inoculation develops a similar but different disease, which, in virtue of its similarity but non-identity, has the power of preventing the development of smallpox, or of at least modifying it to a remarkable degree. Vaccination is thus a very pretty example of homœopathic medication. "R. B. C." states that the vaccine "dose" being a "visible and appreciable quantity, bears no relation to the small dose of homœopathy." After what you said, Sir, in your able leader of last Wednesday, on what constitutes a homœopathic dose, and my remarks in support of your statement, I can hardly understand how a fair opponent can completely ignore it, and calmly repeat his own crude notions of what constitutes a homœopathic dose. Everyone

knows what a minute quantity of vaccine lymph is required to moisten a scratch, and it was pointed out at the time of the epidemic of smallpox in 1865 or 1866, when there was a scarcity of lymph owing to the great demand for it, that it was equally efficacious when diluted to a very considerable degree with glycerine. The actual amount of lymph absorbed in vaccination is far less than the amount which is used to moisten the scratch. The dose, therefore, which is absorbed is an extremely minute one. "R. B. C." goes on to say that "vaccination neither produces smallpox (*sic*) nor cures it." We make him a present of the former statement; but may I remind "R. B. C." that, at the time of the smallpox epidemic to which I have referred, a number of cases were recorded in the medical journals of the treatment of smallpox by vaccination. The result was that (1) many cases were believed to have been much mitigated after the development of the disease, especially if adopted sufficiently early; (2) a certain number of cases seemed uninfluenced; while (3) others were much aggravated. In the latter the vaccine (similar) dose had evidently been too strong for these particular patients, thereby affording an illustration of what homœopaths aim at in the dose—not only to give less than will aggravate, but to give the smallest dose which is curative, and so avoid any risk of aggravation. Of course, "R. B. C." is in happy ignorance of the fact that homœopaths have obtained excellent results in the treatment of smallpox by the internal administration of considerably diluted vaccine. And because Lord Grimthorpe correctly states that vaccination is "a purely homœopathic procedure," "R. B. C." vents his wrath in saying that this view ascribes Jenner's discovery "to a portentous impostor (*sic*), who gave to the world a concoction of nonsense in 1810." How can one argue with a man who thus writes? He goes on to tell us that vaccine lymph contains "living elements which increase the dose by rapidly multiplying within the body," and that the bacteria "permeate the blood and tissues." This is a pure piece of hypothesis and assumption. The relation of these minute organisms to the causation of disease is a point about which there is vast difference of opinion, the current of opinion at present running adverse to the belief in their casual relation. But whether the bacterial theory of disease lives or dies out, the fact remains the same—that a minute dose of vaccine lymph in producing a disease similar to smallpox can both prevent and cure it.

"R. B. C." returns to the charge of the homœopathic pilules, or even tinctures, being non-medicated, and therefore, not what they are stated to be. Mr. Ross, in *The Times* of to-day, indignantly denies such an insinuation, and every homœopathic chemist would speak in the same way. If a chemist's assistant turns out a black sheep, and acts dishonestly to his employers, is that a reason for "R. B. C." insolently charging with dishonesty a class of men who are known to be careful to the minutest detail, and whose whole business interest it is to have in their pharmacies the best drugs which can be prepared? The charge is monstrous and disgraceful. If "R. B. C." knew anything of homœopathic practice, he would be aware that if the wrong medicine is selected, either by doctor or patient, no result follows, while, as soon as the right selection is made, it at once tells. But, accord-

ing to "R. B. C.," this should never be observed, one medicine being as good as another.

"R. B. C." says:—"I am glad to see that no one has attempted to dispute my account of the intellectual barrenness of homœopaths; and this barrenness is as manifest within their profession as beyond it. Medical science affords ample scope for non-contentious work—in anatomy, in pathology, in physiology, in diagnosis. The homœopaths have had nearly a century of opportunity, during which these departments have been assiduously cultivated and rapidly growing; but the homœopaths have done nothing for their advancement. Not one fact, still less one principle, have they contributed to the sum of our scientific progress."

This is simply preposterous. I meant to have noticed this statement of "R. C. B.'s" in my last letter, but I was afraid of making it too long. For a man calmly to say that because homœopaths do not work on the same lines as those of the old school they are intellectually barren is on a par with "R. B. C.'s" other insolent statements. Homœopaths consider therapeutics as their special department, and keep to it. There is no need for them, since this department is almost entirely unworked by the old school, to neglect their own special work in order to do what is admirably done by the overcrowded old school. They are quite content to act on the division of labour principle, allow their brethren of the old school to labour in the only departments of medical science which, with the therapeutical scepticism of the present day, are likely to yield any fruit, and to labour at the ultimate end of medicine, the perfection of therapeutics. The mass of their labours is open to all who will look at it, although "R. B. C.," in his lofty self-esteem, utterly ignores the existence of such labour. The mine of therapeutic rescource, which has been worked steadily and quietly by homœopaths, is being even now largely tapped by our opponents, and the day will come when, in spite of "R. B. C." *et hoc genus omne*, the value of the labours conducted by homœopaths will be recognised. Men will then find, to their surprise, how completely this most important of all fields in medicine has been cultivated by homœopaths, and is within reach of all who will open their eyes. "Not one fact," says "R. B. C.," still less one principle, have they contributed to the sum of our scientific progress." With the greatest principle in therapeutics ever promulgated, and the labours in drug investigation open to him, if he will but look, "R. B. C." calmly shuts his eyes, and then avers that things do not exist.

The only point on which I would think it necessary to notice "J. C. B.'s" letter is his allusion to my statement in regard to Dr. Lauder Brunton. He says "the references made to him are calculated to create the impression that he is half a convert to homœopathy, or at least regards it with some favour." Most undoubtedly, and it is difficult to come to any other conclusion. "J. C. B." tells us that, "by the error of a copyist, the names of one or two (*sic*) homœopathic remedies found their way into the index of diseases in the first edition of his great and admirable work," and that they have been expunged from the second and third editions.

Either "J. C. B." has not seen the Index of Diseases, or he does

not know what remedies do and do not belong to the British Pharmacopœia. To say that "one or two" homœopathic remedies are there to be found is amazing, and, supposing "J. C. B.'s" good faith, we leave him in the above dilemma. He will also find that these remedies are not expunged in the second edition, and if in the third edition (!) Dr. Brunton has found it desirable to overhaul "his copyist," how is it that he still retains *apis*, as a remedy in sore-throat? Has "J. C. B." ever even heard of *apis* as a medicine? The "copyist" must have had unlimited power in compiling the "Index," and if it was his mistake, to be indignantly rectified in the third edition by the author, the occurrence is a very remarkable one.

Finally, I would ask "R. B. C." if, in the face of the imputations of dishonesty, his remarkable ignorance of his subject, and his paltry theoretical difficulties, he will be good enough to ask himself the following questions:—1. Is it in the least probable that about 300 medical practitioners in Great Britain, and 10,000 (the correct figure) in the United States, who have the same education and the same diplomas as their old-school brethren, should be so hopelessly wrong-headed on this one particular point, however sane on others, as not to be able to form a judgment as to the comparative success of two modes of treatment? 2. How is it that, at least in this country, all but the homœopaths of the last five or six years were previously allopaths, and consequently had tested both systems, and gave up professional fellowship and honours for their opinions? 3. How is it that this is the one "heresy" in medicine which has not died a natural death in a few years, but which is yearly increasing, is represented by practitioners all over the world, and in numbers such as no mere mistaken heresy ever could boast of, and is, despite "R. B. C.'s" denials, fast leavening the ranks of their opponents? 4. How is it that, while scepticism in regard to the action of medicine in the old school is notorious, homœopaths are staunch believers in their own treatment? 5. How is it that fully half, if not more, of the symptoms produced by drugs on the healthy body are of no therapeutic use whatever, except on the homœopathic principle? It is this disheartening discovery that prevents the prosecution of investigations as to the pure effects of drugs on the healthy body—both schools agreeing that this is the only reliable mode of investigation—while on the homœopathic law every drug-symptom elicited becomes a key to its therapeutic use.

D. DYCE BROWN.

A SCEPTIC.

[*Jan.* 12.

It is an amusing controversy about the *odium medicum*, but what I, as a sceptically-minded layman, find most amusing is, first, the proof so abundantly and so unconsciously supplied by the orthodox controversialists of the existence of that *odium* which they disclaim; and, second, the splendid audacity of their pretensions on the score of medical knowledge and pathology. No one reading the tremendous diatribes of "R. B. C." and "J. C. B." can have the least idea what very nice, modest, candid fellows doctors are in private life. As soon as you convince them that you cannot be bounced or bluffed by claims

to medical knowledge transcending ordinary faculties, they become quite confidential and humble, admit that their so-called science is only more or less clever guess-work, and bemoan the impossibility of making substantial advance. But in public, as your readers see, they talk in a style calculated to persuade the simple that medicine is an exact science, marching with the inexorable step of mathematics upon the fast-diminishing realm of the unknown.

I have just opened at random "Ringer's Handbook of Therapeutics." It opened at *Nux Vomica*, and as that is a drug used in both schools it is interesting to learn what medical knowledge about it comes to. Ringer, be it observed, is a doctor just about as good as they make them. He has got all the pathology, and so forth, at his fingers' ends. He can tell you what happens when you inject a medicine, and cut this or the other nerve, or tie up this or the other duct. When a man or frog dies under treatment Ringer knows which nervous centre was affected by the poison. I opened his book at page 553, right in the middle of *nux vomica*, and the first thing I read was this:—"The tincture or extract of *nux vomica* has long been employed to correct constipation, habitual or temporary." This looked like knowledge, and I went on cheerfully. Presently I read—

"As our knowledge of the action of *nux vomica* in its relation to constipation is at present imperfect the results appear to be capricious. It is as well, therefore, not to be too sanguine of success; for in some cases it answers beyond all expectation, while in other, apparently similar, cases it completely fails."

Is not this a perfectly lovely specimen of "science"? *Nux vomica* is an exceedingly well-known drug, constantly in use either under its own name or as strychnia, which is its active principle. Constipation is one of the most familiar of all maladies, offering boundless facilities for the exercise of a scientific method. Ringer is one of the persons best equipped, by knowledge of pathology on one side, and of drugs on the other, to apply the right drug with the unerring exactitude of science. What does it all come to? Why, just this, that the exponent of science, the master of that "medical knowledge" so arrogantly vaunted by your correspondents, dealing with a common-place ailment and an every-day drug, knows no more what that drug will do for a given patient than the first old woman he might meet in the street. He will try it and see what happens—that is all. Why, I could do as much for myself, at any rate, with the aid of one of the homœopathic manuals that excite the derision of lofty scientific spirits.

However, there are weak points in every system, and as *nux vomica* might not be Ringer's "strong suit," I turned to his index in order to see what else he could do for constipation. I found no end of things recommended—aloes, belladonna, Carlsbad water, coffee, cascara, croton oil, jalap, rhubarb, senna, guaiacum, magnesia, sulphate of potash (sometimes poisonous, but you can only find out by trying whether it is so or not in a given case), mercury, oranges, and ever so many more. I turned up a number of these remedies, and searched in vain for a cure for constipation. These drugs, most of them quite familiar to the old woman before mentioned, are only laxatives or purgatives. That is to say, they relieve constipation, but do not remove the condition on which it depends, any more than baling out a

boat stops the leak in its bottom. Ringer does not know any cure for constipation, and he does not know, except by direct experiment upon the patient, which is the best palliative in any given case. They call this sort of thing science, and medical knowledge, and pathology, and Heaven knows how many other fine names! On the strength of this blundering guess-work they come down in thunders of indignation upon the poor homœopath who, at worst, only adds one more guess to the pile, and whose stuff, to take their account of it, is at least in no danger of turning out poisonous like their sulphate of potash. Can you conceive anything more ludicrous than this fine Ephesian rage?

Turning over the leaves, I came, at page 386, to nitrite of amyl, about which I read—"In thirty or forty seconds, whether inhaled, subcutaneously injected, or swallowed, it flushes the face and increases the heat and perspiration of the head, face, and neck." Then comes a lot of pathological discussion as to how it does this. How a drug acts is always a great question, and people like your correspondent "J. C. B." are full of scorn for practitioners who are content to know that it does act in a particular way. But here, again, these doctors are excruciatingly funny if you only follow them up. After all their pathology they generally leave it quite an open question how a drug acts, or if one is cocksure that it acts in one way you may be very certain that another will not be long in carving up a few frogs to show that it acts in quite another way. As to nitrite of amyl, Ringer says it "must act either on the vaso-motor nerve-trunks, or on the muscular coats of the arteries." Brunton believes "it partially paralyses the sympathetic ganglia and their motor nerves." Here exact science offers you three alternatives, and at the same time condemns as pernicious heretics the men who say they can do without settling the question. Could anything be funnier than this pathology ending in a quagmire and accompanied by a dogmatism which holds up to scorn men whose only offence is that they wait for the orthodox to make up their minds? But having had enough pathology I turned over a page or two and lighted upon this passage—"Nitrite of amyl will prevent or greatly lessen these flushings or heats," which have just been described and precisely resemble the symptoms already quoted as following inhalation, injection, or swallowing of amyl nitrite. What is this but curing by similars? A little farther on I find that Ringer has discovered, like the homœopaths, that when like cures like a very small quantity of the drug must be given. He finds the thirtieth part of a drop sufficient, and in one case a great deal too much, for it threw the patient into "a trance-like state," with the "breathing becoming rather panting." Here, then, we have the homœopathic rule and the homœopathic dose, yet nobody excommunicates Ringer. Homœopaths, I believe, use amyl nitrite in the same way and for the same symptoms and in the same doses, but they are knaves or fools for their pains. What is the difference? Just this—the homœopaths say they choose the drug because from its effect on the healthy they feel sure it will cure heats and flushings; and the orthodox find it out somehow, and then write pages of contradictory twaddle about how it acts, which they call pathology.

Doctors would do well to get it into their heads that a patient wants

to be cured, and does not care a brass farthing how his doctor lighted upon the proper drug. Pathology is a nice amusement for the doctors, because it enables them to string their guesses together and call them theory or science, and furnishes material for learned and inconclusive papers to be read to their societies. But a great deal that goes under the name of pathology, and just that part of it that the paper-reading part of the profession are most proud of, is of no earthly use to the patient. It enables a man to talk grandly about how a disease acts, and to interpose a number of secondary causes between the primary cause and the effect. But it does not tell him why the disease is there; it does not give him a grip of the primary cause, or tell him how to meet it. Anything he happens to know of that branch of the subject has been found out empirically—that is, by trying experiments upon patients as Ringer does with his nux vomica. Homœopaths try experiments too, and they say with success. They say, moreover, that their rule gives them some clue to the right drug and some power of prediction. That may or may not be, but when everybody is groping in the dark their clue can do no harm; and, oddly enough, the more medical reading one does the more examples turn up of agreement in practice between people who abuse one another's theories.

One more point. It is all very fine for a few gentlemen in London to talk big about a "method which rests upon the intimate nature of morbid action." I have shown by one or two instances how much their knowledge comes to, and how far it helps them forward; but, at any rate, they can always reel you off the possible explanations of secondary causes. Take the profession at large on the other hand. Take the general practitioner who does the great mass of physicking all over the country. What does he know or care, as a rule, about pathological theories? Just nothing at all. He got a certain amount of pathology at college, and he is content. All the rest he gets from experience, and he can get it from no better source. But observe that every homœopathic doctor gets just the same equipment from the same teachers. He knows all the pathology that serves the great mass of the profession, and what he neglects is merely this fine talk by a few clever gentlemen who cannot agree among themselves about vasomotor centres and sympathetic ganglia.

How, I wonder, could "R. B. C." write without a smile about progress depending upon intimate knowledge of morbid action? He knows well enough that there is no such progress. A great physician is like a great artist, and other men cannot catch the trick. They try very hard. They copy his prescriptions, and they use his drugs, thereby causing as many changes of fashion in this exact and progressive science as in ladies' bonnets. But they cannot get his results, and so in their private conclaves they call him a humbug and ascribe his success to anything except his capacity to cure his patients. A physician must know the human body, and he must know the properties of drugs, just as a sculptor must know the human body and the properties of marble. But this equipment being given, it is art, not science, that makes a man great in one line or the other. Just as the great sculptor is endowed with an exquisite perception of form, so is the great doctor endowed with an exquisite perception of his patient as a whole. He instinctively grasps the medical form—

the thing which, I suppose, homœopaths try to get at under the name of totality of symptoms. All this theorising about the intimate nature of morbid action does not enable any man to stop morbid action. They have tracked cancer down to its cellular home. They can show you drawings and preparations illustrating every step of its operation, and they can detect cancer cells in the merest fragment of tissue. But they cannot tell you why the spindle-shaped cell makes its appearance, nor can they stop its growth. The secrets of life and death, of heredity and habit, lie far beyond the utmost reach of our finest appliances, and an atom of protoplasm in which our most powerful lenses can discover no trace of structure holds enfolded within itself the history and the future of a race.

A SCEPTIC.

DR. GUTTERIDGE.

[*Jan.* 12.

On reading that "by the error of a copyist one or two homœopathic remedies had found their way" into Dr. Brunton's "Index of Diseases," I at once reached down my copy in order to verify this statement. I found that aconite was very extensively advised in fever and feverishness, that belladonna was very frequently mentioned, and such other well-known names as arnica, bryonia, pulsatilla, cimicifuga, gelsemium, chamomilla, ignatia, sanguinaria, chimaphila, hydrastis, rhus toxicodendron, veratrum album, hamamelis, veratrum viride, phytolacca, and apocynum, together with apis mellifica grindelia, viola tricolor, and thuja, were to be found as I looked through that alphabetical arrangement. A very good two or three it must be admitted, especially as they are mentioned and prescribed for on distinctly homœopathic lines, as I will presently show in detail. For instance, in measles we are directed to aconite, pulsatilla, antim. tart., and veratrum viride; in scarlet fever, to aconite, arsenic, belladonna, mercury (in $\frac{1}{8}$-grain doses), rhus tox., and veratrum viride; in sore throat, to aconite, belladonna, hydrastis, ipecacuanha, mercury, nitric acid, phytolacca, podophyllum, pulsatilla, sanguinaria, and veratrum viride; in quinsy, to aconite, apis, arsenic, belladonna, mercury, and phytolacca; and, finally, in lumbago, to aconite, cimicifuga, and rhus. I give these only as examples to prove how thoroughly different to ordinary practice the treatment indicated is, and how thoroughly homœopathic. I would submit that as the index in which all this is found is part and parcel of Dr. Brunton's work, so he, and he alone, must be held responsible for it, and further that the directions must be taken to be thoroughly reliable, the homœopathic as well as the orthodox, though both indiscriminately follow each other, or the whole should be put aside as the mistake, and a very dangerous one too, of this said copyist. I know full well that Dr. Brunton is not a homœopath, and, further, that personally he has no leaning that way, still it is only fair to the public for them to know, not to how small, but to how large an extent one of our chief medical examiners is willing without intimation or acknowledgment to borrow from his ostracised brethren.

Homœopathy has done something for the good of mankind, since it has presented us with an array of new medicines like those I have

referred to. It has taught the value of one remedy at a time, of a small and frequently repeated dose, it has demonstrated the specific action of drugs, it has proved the existence of the law of "like curing like," it has shown that medicines need not be nauseous, and that it, at all events, can treat disease *cito, bene, et jucunde.* I have tried it "among all sorts and conditions of men," from the pauper to the prince, and in nearly every disease that flesh is heir to, and especially in cancer and consumption, and I am very well satisfied with its results.

R. S. GUTTERIDGE, M.D.

58, *Brook-street, Grosvenor-square, W.*

THE FACTS OF R. B. C.

[*Jan.* 12.

Allow me to point out an extraordinary mistake in arithmetic in the letter of "R. B. C." in *The Times* of to-day. Your correspondent states that the earth contains approximately 4,000,000,000 (four thousand million) of cubic feet, and founds an argument on these figures. But the earth is nearly 8,000 miles in diameter, and a sphere of that size contains in round numbers 250,000,000,000 (two hundred and fifty thousand million) cubic miles, while each cubic mile contains about 130,000,000,000 (a hundred and thirty thousand million cubic feet). I fear the doctors had better leave figures to another profession.

F. G. S.

J. C. B. (2).

[*Jan.* 14.

Retreating, but fighting still, Lord Grimthorpe gives up vaccine lymph and falls back on "the hellish oorali," as the Laureate calls it. One of his allies seems disposed to defend the vaccine position a little longer, and argues as follows:—Vaccinia and small-pox are one and the same disease; the proper treatment for small-pox is a little more small-pox (just as the treatment of vitriol poisoning is, I presume, according to such wiseacres, a little more vitriol); and the man who has the small-pox ought, therefore, to be well vaccinated through his stomach or skin, which is a strictly homœopathic procedure. But Lord Grimthorpe prefers to instance oorali, or the arrow poison, in the action of which, he thinks, with his incorrigible analogical perversity, he has discovered a resemblance to the action of a homœopathic remedy. "Do the allopaths," he asks defiantly, know "how it acts?" Thanks to the labours of Claude Bernard and others, they know very well how it acts, but does his Lordship, without levity, maintain that the fact that certain poisons kill with definite symptoms when introduced into the blood in small but appreciable quantities gives plausibility to the view that other poisons when administered in infinitesimal doses will cure diseases which are mimicked in a few symptoms by their actions when given in large doses? Oorali is the extract of a plant which the natives of Demerara and the valley of the Amazon used to smear freely on their arrows. The lethal dose of it is not a decillionth of a grain or any infinitesimal phantom, but one grain,

and as much as half a grain has been given medicinally. Now what Lord Grimthorpe has to do in order to make oorali serve his purpose is to show that in infinitesimal doses it is useful in the treatment of paralysis, for its "provings" are of a paralytic nature. But this will probably puzzle him, and in the meantime it has been shown that in ordinary doses producing a physiological effect it is useful in an exactly opposite condition—namely, tetanus or lock-jaw.

Two grains of strychnine will infallibly kill every man, woman, or child to whom it may be administered, and so will an eighth of a grain of digitaline—and these are small doses no doubt, but they are a whole universe greater than a decillionth of a grain, are followed by symptoms which "who runs may read," and are capable of recognition by the chemist. A fine needle introduced into the pithing point will kill. That is a small dose of cold steel, but because it is effective, and, like Mercutio's wound, "enough," are we to infer that an ultimate atom of gold will cure ophthalmia?

"Then how about stinks," continues Lord Grimthorpe, with customary emphasis, "stinks, a single whiff of which sometimes poisons people, while at other times the same or other people safely live in them for hours?" Well, stinks are due to odorous particles or gases coming in contact with the mucous membrane of the nose, and stimulating the nerve cells there, and stinks, as stinks, are sensations and nothing more. But with the stinks there may be living organisms—disease-bearing bacteria or spores—which entering the blood by the nasal or pulmonary mucous membranes, plant themselves and breed, and cause illness where the conditions are congenial to them (and hence some folks are poisoned by stinks), or perish where the conditions are unpropitious (and hence some folks inhaling the same stinks are not a penny the worse); or with the stinks there may be poisonous gases, like sulphuretted hydrogen or carbonic acid, which will have the same deleterious effects on all who breathe them. Lord Grimthorpe has been already convinced that there is nothing homœopathic in the action of micro-parasites, and he will be readily satisfied that neither is there in the action of gases, poisonous or innocent. Has he ever taken chloroform? A quarter of an ounce of that fluid, not a very homœopathic dose, had probably to be vapourised and inhaled before he became unconscious. All poisonous gases kill in quantities which can be accurately measured and determined by the chemist, and many of them cause visible changes in the solids and fluids of the body, but who shall gauge a decillionth or track its path? Has Lord Grimthorpe reflected that every time he enters a druggist's shop or walks past one he inhales countless decillionths of at least a hundred powerful drugs? Why waste his money on globules then? Why not sniff and be cured? I have heard of a man for whom a homœopath prescribed a decillionth of a grain of camphor. But he was in the habit of protecting his clothes against moths by that solid volatile oil, and every time he went to his wardrobe took in millions of decillionths of it. Where, then, was the virtue of this extra one decillionth, unless it be that decillionths are sticklers for etiquette, and will not close with a disease until they have been ceremoniously introduced to it in globule form?

Dr. Millican, who is making rapid progress in his homœopathic

education, drags once more before us that time-honoured grain of musk, and would have us believe that because minute particles can produce a sensory thrill still more minute particles can cure disease. He might, however, have taken up a stronger position, and have sought a parallel for his drug atoms, not in the gross emanations of odorous bodies, but in the subtle modulations of light, the violet rays of which enter the eye at the rate of 699,000,000,000,000 in a second. And even light waves are coarse when compared with homœopathic remedies, for it has been calculated that these rays, travelling at the rate of 192,000 miles a second, would take millions of millions of millions of years to pass from one end to the other of one grain of sulphur, laid out in decillionth globules at 20 to the inch. And a decillionth is homœopathically a gigantic dose, for Hahnemann latterly employed the olfaction, or, in plain language, smelling of decillionth globules. Putting two globules in a phial containing two drachms of alcohol, mixed with an equal quantity of water, he caused this to be inhaled once or twice with each nostril in acute and chronic diseases. An apt disciple was the lady of whom Simpson tells, who, having been subjected to this process of the administration of a homœopathic dose by olfaction by her doctor, passed the fee before his nose and then replaced it in her pocket.

The letters which Lord Grimthorpe takes upon him to answer he does not apparently take the trouble to read, for he now protests that not a word has been said in answer to his tremendous citation of leading authorities in the profession who have spoken disparagingly of medical science and work, whereas he was distinctly told that what these authorities condemned is the fast disappearing empirical element in medicine which is common to it and homœopathy. If their censures on medical science are good for anything, equally good must be their censures on homœopathy, which are tenfold stronger. But it is past finding out what his Lordship would be at, for in one place he says that there is no driving "into allopathic heads, English or Scotch," that their infallibility is in question, and in another, a few lines further on, pours withering disdain on them for confessing their own incompetency. The quotations which he has adduced are scraps, riven sometimes from a context which would have put a different complexion on them, in the writings of a paradoxical philosopher, who is seldom to be taken quite seriously, and of others who have dwelt at times on the backwardness which must always fret ardent students in every progressive science. It may be taken for granted that Lord Grimthorpe never wasted a qualm on the mode of practice at the Parliamentary Bar, now threatened with extinction, under which a barrister may hold as many as 10 or 12 briefs for one day of four hours, and receive enormous fees for undertaking work which he cannot possibly perform.

Empiricism is for Lord Grimthorpe the one criterion in medicine. Significant discoveries as to a connection between the chemical constitution and physiological action of drugs, investigations into selective affinities of drugs for certain tissues and organs, experiments as to their influence on the various functions of life and the processes of disease, have no interest for him. All he wants to know is—What did So-and-so good when he was ill as I am, or thought

he was? Bone-setters and rubbers are his surgical advisers, and a study of the advertisements of the quack medicines ought to be his guide to the selection of a physician, and he is somewhat to be pitied if he has to balance their conflicting claims. The British Parliament, made up of men like him, when it had never granted a farthing for the advancement of science, once voted £5,000 for the purchase of Mrs. Stephen's specific against certain diseases, which was found, when the prescription was handed over, to consist of "egg-shells and snails' shells, with the snails in them calcined, ash-keys, hips and haws, swine-cress, and various other vegetables all burned to a cinder, with camomile flowers and fennel." And not a whit more idiotic was Mrs. Stephen's specific than a number of prescriptions that might be culled from homœopathic books—prescriptions in which Lord Grimthorpe is bound to believe, as all of them are certified to have done people good. One of the most recent advances in homœopathy is the discovery, just published in a homœopathic journal, that anacardium is good for profane swearing. Given to healthy men it causes blasphemy; but to those who have contracted a habit of swearing, infinitesimal doses of it are corrective. It was very soothing in the case of a clergyman, who had knocked his nose against a door in the dark, and forgotten himself accordingly.

Homœopathy has been before the medical profession in this country for nearly eighty years. Its literature has been open to all, and all have been free to test for themselves its much be-lauded remedies. The general practitioner, one of your correspondents, who is in agreement with Lord Grimthorpe, avers he cares nothing for pathological theories. He wants in these fiercely competitive days to cure his patients and achieve a reputation for skill, and he is not likely to neglect any means that may aid him in so doing. Is it not conclusive, then, against homœopathy, to anyone capable of weighing evidence, that 22,669 medical men out of a total of 23,027, according to the latest returns in England, Scotland, and Ireland, absolutely reject it as a mixture of twaddle and trickery, while only 258 accept it more or less? And the number of homœopaths is dwindling year by year, and so is the number of homœopathic druggists.

But doctors, proceeds Lord Grimthorpe, will not examine the proofs of homœopathy. They have done so again and again, and found them wanting. They will not, he grumbles, discuss its published statistics. They have done so repeatedly, but they may be excused if they dismiss summarily what he calls statistics, and which consist of second-hand versions of the rates of mortality in a hospital in Vienna, and half a hospital in Paris, which he does not even name. I have already shown him that, taking his figures as he offers them, they are unfavourable to homœopathy, and I would here add that if they are drawn from the returns of Dr. Fleischmann, of the Homœopathic Hospital of Sisters of Charity at Vienna, then they have been reduced to mincemeat more than once. It would take more space than you, sir, could allow me, to reproduce the destructive analysis of these figures, which has long been before the medical world, and I shall only repeat the conclusion arrived at, after a careful survey of them, by Professor Gairdner, of Glasgow, than whom there is no more candid or open-minded man. "I maintain," he said, "without fear

of contradiction, that the homœopathic returns are not only without all triumph to the system, but that they cover it with disgrace."

The case in medical ethics stated, and very lamely stated, by Lord Grimthorpe, scarcely admits of judgment being passed on it, because the facts are not sufficiently fully set forth. A regular practitioner, who declared a lady to have cancer, was superseded in his attendance on her by a homœopath, who declared she had not cancer, and the lady's father then asked the regular practitioner to examine her again, and it is certainly curious that he should have sought further assistance of one who had blundered so egregiously. But in deciding whether the regular practitioner was justified in declining the father's invitation, we must know more than we do of the lady's situation. If she was living in a country place, where other medical advice was not available, then he was wrong—utterly wrong—in withholding from her in her suffering whatever assistance it was in his power to offer, and I take leave to doubt that Lord Grimthorpe and his "person whose name would command attention" are able to show that any regular practitioner has acted in such a way. But if she was residing where medical skill was plentiful, then he was well warranted in declining to have anything more to do with the case, and in saying to the patient's father, "As you have discarded me for a homœopath, whom I look upon as a quack, and have been informed that my diagnosis was wrong, I think you had better call in someone else. I cannot carry out homœopathic treatment, which I believe to be nugatory, and your daughter and you will probably have more confidence in someone who comes to the case unbiassed." Is a doctor so poor a thing that he is to be allowed no remnant of *amour propre*, but is to be buffeted and insulted at pleasure by every "person whose name would command attention"? What would Lord Grimthorpe have done if, when he was at the Bar, a Parliamentary agent, who, annoyed by his oversights and provocations to the Committee, had taken his brief from him and given it to another, had come to him after a lapse of a day or two and said, "Here, I wish you to take your brief again, and go on with the case?" Would he, bending low,

> "And in a bondman's key,
> "With 'bated breath, and whispering humbleness,"

have accepted it with thanks, or would he——? Well, I prefer not even to imagine the other and more probable alternative. But what in Lord Grimthorpe would have been becoming dignity and self-respect, in a doctor was disgraceful and murderous trade-unionism. Let Lord Grimthorpe note that it was the daughter of a "person whose name would command attention" whom the practitioner declined to see, and that it must have required some loyalty to principle in a struggling man to resist the temptation to resume his attendance on her and pocket the affront. Did he ever hear of a practitioner who refused to see a beggar or pauper because he had been to the homœopaths?

Lord Grimthorpe suggests that I have taken an unfair advantage in repeating again his erroneous statement that there are 10,000 homœopaths in England, whereas the actual number is 258 (not 278 as stated in my last letter). He says this slip of the pen has been w ice corrected. I have seen no correction of it but my own, and it

is not a little remarkable that, after that correction of mine, which appeared in *The Times* of January 4, Lord Grimthorpe should have written a letter to the extent of a column and a half, which appeared in *The Times* of January 6, without withdrawing or explaining it. He tells us now that it was "a clerical error transparent on the face of it," and that when he wrote "England" he meant the "world." How comes it, then, that this scrupulous controversialist tells us in his letter of January 6, that there are 11,000 homœopaths in America? Eleven thousand in America and 10,000 in the world! And the fact that he had made this computation for America was clearly before him when he penned his letter which appeared in *The Times* of January 12, and explained that by "England" he meant "the world," for it is the foundation of the paragraph in my letter on a criticism of which he ventures to impugn my honesty. Let him look to his own, and tell us what he thinks of a debater who can condescend to such "rubbish," for I shall not use a stronger epithet than he does.

But Lord Grimthorpe's "clerical error," although unworthy of a vicar-general, and more suitable to a slipshod scientist like Mr. Proctor, is, he retorts, a bagatelle beside an aberration of mine.

"Not one or two," as I have said, but "many homœopathic remedies have found their way into Dr. Lauder Brunton's *Materia Medica*," and Dr. Dyce Brown, assuming that I write in good faith, which he thinks doubtful, supposes that I have not seen the index of diseases in that book. Now, I have not attributed dishonesty to any individual homœopath; indeed, I have always maintained that there are many warp-brained and weak-minded members of the sect, and Dr. Dyce Brown would perhaps be well advised not to challenge personalities. But the benevolent construction which he puts on my "amazing misstatements" is politely declined. I stand to my guns. Dr. Lauder Brunton, who ought to know, says in the preface to his third edition (p. 9), which Dr. Dyce Brown has read, that there is included "in the Therapeutic Index one remedy which the homœopaths claim as theirs. . . . To the best of my knowledge," he goes on, "this is the only remedy I have taken from a homœopathic source." Insinuating that Dr. Lauder Brunton has unjustifiably sheltered himself behind the error of a copyist, Dr. Dyce Brown asks why "apis" is retained as a remedy for sore throat in the third edition of his book. It is not so retained, and Dr. Dyce Brown ought to spare us the trouble of correcting such gratuitous blunders. And what, it must here be asked, constitutes a homœopathic remedy? Not simply the fact that it is used by homœopathists. They use aconite, which was known to Dioscorides and Galen; *nux vomica*, which we probably owe to the Arabian physicians; and lead, which was a tonic amid the Greeks and Romans. Not simply the fact that it is used in small doses, for small doses of poisonous drugs were necessarily in vogue long before Hahnemann had made his first venture in humbug by selling his quack borax nostrum; and what is called a small dose by a doctor, as a drop or grain, has about the same proportion to a homœopathic dose that the solar system has to a mote in the sunbeam. That remedy can only be called homœopathic, our knowledge of which we owe to homœopaths, or which is employed in homœopathic attenuations.

Now, no drug is employed by doctors in homœopathic dilutions, and I cannot recall one of the slightest utility, and in actual use, which a homœopath has primarily introduced to us. Eager as doctors are for fresh means of combating the terrible complexity of disease, they will ask no questions as to the parentage of a drug that comes to them with healing on its wings. Mr. Martindale's list of extra-pharmacopœial remedies shows how little they restrict themselves to the official list, and it is therefore extraordinary that the homœopaths, whose whole work has been in therapeutics, have given us not one drug worth having. Their toils have been sterile as those of the lads and lasses who engage in the Hallow-e'en pastime described by Burns as "winning three weights o' naething," and who, as the reward of their industry, have sometimes the satisfaction of seeing an illusion. False principles and faulty methods have crippled all their efforts, and the greatest achievement which one of your sympathetic correspondents can claim for them is Dr. Dudgeon's sphygmograph. Why, one might suppose that Dr. Dudgeon, and not Vierodt, had invented the instrument, while all he has done has been to make some infinitesimal mechanical alteration on Pond's American sphygmograph, thus producing an instrument which Dr. Burdon Sanderson* and Mr. Hawksley (and where shall we find higher authorities?) regard as most untrustworthy. The clinical observations of Henderson, to which Dr. Dudgeon refers, were made before he strayed into the Hahnemannian quagmire. When young Toots began to have whiskers he left off having brains, and when Dr. Henderson became a homœopathist he ceased to contribute usefully to medical science.

The cloudy difficulties of a sceptic would be best dissipated in a medical journal, for it would be manifestly inconvenient to discuss with him constipation and the actions of drugs in the columns of *The Times*. He seems to think that medical science is synonymous with therapeutics, and forgets all about anatomy, physiology, vital chemistry, and pathology, as, with the mischievous propensities of a schoolboy of tender years, he seeks out what he thinks will be hard nuts for the doctors to crack—hard nuts which are rotten and hollow, every one of them.

Dr. Dudgeon is too shrewd in his guesses, and I can only hope that he is more successful in seeing through the mystery of disease than in piercing the veil of anonymity. J. C. B.

DR. A. DUPRE.

[*Jan.* 14.

To argue with a believer in homœopathy about homœopathy is as profitless an undertaking as to argue with table turners, spiritualists, and earth-flatteners, and I am not inclined to essay the task. I should, however, like to ask Dr. Millican the following question:—Why was it found necessary to bring the attar of roses all the way

* In a letter to Dr. Dudgeon, dated Jan. 18, Dr. Burdon Sanderson says: "I am not aware that I have ever been asked my opinion on the merits of your instrument, and I do not think that I have ever expressed any opinion unasked. I do not know sufficient about it myself, but know that it has been found by others a satisfactory instrument."

from Constantinople? If Dr. Millican had merely asked a friend to spill a drop of the attar at Constantinople, this would, on homœopathic principles, have been more than sufficient to scent his house here in London for years after.

The main object of this letter, however, is to give some facts regarding homœopathic pilules which may show the sensible portion of the public on which side unbounded credulity is to be found.

In 1873 I examined, for my friend the late Dr. Anstie, a number of homœopathic pilules obtained from houses well known in London and Liverpool. The results were published by Dr. Anstie in *The Practitioner*, vol. x., page 254, and vol. xi., page 55. Those specially interested will find in the latter volume the names of the houses from which the pilules were purchased. The dilutions chosen were what are known as the first and second, these being the only dilutions which are at all within the range of chemical analysis.

The following, shortly stated, are the results of the analyses:—

Sulphate of copper pilules, second dil.—First sample—No copper could be detected in 100 pilules. Second sample—No copper could be detected in 200 pilules. If in the second case as little as 1-2,000,000th of a grain of sulphate of copper per pilule had been present it would have been detected.

Corrosive sublimate pilules, second dil.—Two samples.—It was just possible to detect mercury when 200 pilules were taken for analysis. Less than 1-400,000th part of a grain of corrosive sublimate per pilule was present.

Nux vomica pilules, second dil.—No strychnine could be detected, even when 300 pilules were taken.

Belladonna pilules, second dil.—No atropine could be detected in 300 pilules. If as little as 1-2,000,000th of a grain per pilule of either strychnine or atropine had been present it would have been detected.

Aconitum napellus pilules, first dil.—First sample—No aconite could be detected in 100 pilules. Second sample—No aconite could be detected in 100 pilules. If as little as 1-800,000th part of a grain of aconite per pilule had been present it would have been detected.

Belladonna pilules, first dil.—First sample—No atropine could be detected in 100 pilules by chemical means. Second sample—No atropine could be detected in 100 pilules by chemical means. If as little as 1-600,000th part of a grain of atropine per pilule had been present it would have been detected.

The above facts were published in *The Practitioner* in the year and volumes before mentioned, and, as far as I am aware, they were never called in question.

It will be seen that the only instance in which I could at all detect the presence of the active ingredient was that of the corrosive sublimate pilules. In all the other cases I failed to detect the active ingredient professedly present, and, although I do not wish to affirm that these ingredients were entirely absent, I have given the quantities more of which they cannot have contained, and they are infinitesimal doses. Yet, according to homœopathic notions, even these quantities are exceedingly large. These pilules represent the weakest kind used —*i.e.*, those containing the maximum amount of drug. To render

them really powerful they would have to be diluted many millions of times.

It is but fair to state that the method adopted in the manufacture of at least several varieties of the pilules satisfactorily explains some of the statements made in "R. B. C.'s" letters, which seem to throw doubt on the honesty of the dispensers of these pilules.

The pilules are, in the first place, made from pure milk sugar, and are then individually moistened with the desired amount of a tincture containing 1-100th, 1-10,000th, &c., of a grain of a so-called mother tincture of the drug required.

I leave the public to draw their own conclusions, and remain

A. DUPRE.*

Westminster Hospital.

R. B. C. (2).

[*Jan.* 14.

Professor Michael Foster, who is quoted by Mr. Millican, is an eminent physiologist, but eminent men have before now carried on customary statements from text-book to text-book without verifying, or perhaps without even noticing, them. There is no evidence that the sense of smell is excited by detached particles; what evidence there is and all analogy point to an opposite conclusion. We know that an odorous substance may incite the sense of smell for an indefinite time without appreciable loss of weight, and hence, in all probability, without diminution of substance. We have reason to believe that all conduction through nerves is the same, however the nerve may be excited, or whatever results to consciousness may follow from its excitation; and we know that these results depend neither on the nerve nor on the stimulus, but solely on the endowments of the brain centre, to which the impression is conveyed. Every impression made on the optic nerve is felt as light; every impression made on the nerve of hearing is felt as sound, and so on. The sensations which we know best, those of sight, hearing, and warmth, are not excited by particles (although light was once supposed to be particulate), but by movement, originating in external matter, and propagated to the nerves concerned through air or ether, and ultimately through covering tissue. The case of smell is, in all probability, the same; and one reason for thinking so is that it can only be excited through a gaseous medium. If the nostrils be filled with water, even scented water, no smell can be perceived; and it may easily be understood that water would be likely to quench delicate vibrations. In any case, the possible minuteness of odorous particles does not affect the measurable magnitude of the particles which are obtained by the use of a pestle and mortar. In my argument that the supposed homœopathic dose would often be smaller than anything we can conceive of the magnitude of the ultimate particles of matter, I took from a publication by Sir James Simpson the statement that the earth contains 4,000,000,000 cubic feet. I did not verify the statement, and "F. G. S." says it is wrong. If so, the error is not large enough to affect my argument. The figures given by "F. G. S." leave a probable ultimate minuteness which does

* For replies to this letter see Appendix.

not much fall short of being four times the size of that favourite dose the quadrillionth, and which exceeds the size of a decillionth by thirty-eight places of figures. Dr. Dudgeon says that Hahnemann says that the itch insect is a kind of mite, and that Wichmann had given a drawing of it. Many people gave drawings of supposed itch insects; but the patients from whom the insects were taken were probably hospitable to more than one variety, and the work of Renucci showed all previous drawings to be incorrect, and to have been made from wrong originals. R. B. C.

DR. J. H. CLARKE (2).

[*Jan.* 14.

In all discussions about the powers of different doses of medicines one fact of vital importance is almost invariably left out of sight—namely, that there is no definite relation between the size of a dose and the size of a patient. Two patients, the one being twice the weight of the other, may take the same medicine in the same dose, and no one can tell beforehand whether the larger patient may not be ten times more powerfully affected than the other. It is true that a medical man of eminence, Dr. Lauder Brunton, did once gravely say that it would be "more exact" to weigh the patient before prescribing the dose, though it would be inconvenient. But Dr. Brunton belongs to the school of drug students who think that the best way to find out how drugs will effect men is to give them in poisonous doses to animals under all sorts of abnormal conditions. These experimenters are always careful to note the weight of the animals they poison, and the colour of their coats, as well as the weight or measure of the dose. This looks very scientific and exact, but really it is very unscientific, for the reason that no one can tell whether a large dog will not be affected as powerfully as a small one by an equal dose of a drug, or whether a black-and-tan dog is more susceptible to its action than a white one. And the idea of weighing a patient before dosing him is, for the like reason, as ridiculous as it looks.

Susceptibility to drug action depends on many things, but scarcely at all on mere weight, It is of two kinds—constitutional susceptibility and induced. There are some people who never can endure certain drugs, even in the minutest quantities. We call this "idiosyncrasy," but we cannot explain it. When the health is deranged it is found by experience that there is induced a temporary susceptibility to the influence of those drugs which are capable of producing similar disturbance in the healthy. For example, aconite, in some of those who have taken it while in health, has caused inflammation in the eyes, but not in everyone, since all are not susceptible. But if the eye has become inflamed, as after an operation or an accident, then it is much more susceptible to the action of aconite than when in health, and every person taking it in these conditions will experience its effects. This is induced or temporary susceptibility. To argue about the size of doses as if they were an abstract quantity, without taking into consideration individual peculiarities or the conditions of disease, is unscientific and absurd.

The power of physically minute quantities of substances to effect great changes in animate things is not a question of astronomy, of

physics, or of chemistry, but one of physiology, and is to be decided by observation and experience, and in no other way. Darwin showed in his work on "Insectivorous Plants" that one 20,000,000th of a grain of phosphate of ammonia was sufficient to produce a distinct effect on the glands of *Drosera rotundifolia*. He says further: "When a dog stands a quarter of a mile to the leeward of a deer or other animal, and perceives its presence, the odorous particles produce some change in the olfactory nerves; yet these particles must be *infinitely smaller* (italics mine) than those of the phosphate of ammonia weighing the 20,000,000th of a grain." It would be just as easy to prove by astronomy, physics, or chemistry that this is absurd and impossible as it is to prove that the infinitesimal doses of the homœopaths are of no effect. Experience alone can decide in either case. The size of the animal has little or nothing to do with it. It would not be a bit more "exact" if we knew the weight of the dog that scented the deer at a given distance; and a small dog might be less keen to discover it than a large one, though the latter received a proportionally smaller dose of the scent.

Metallurgy also furnishes evidence of the power of minute quantities. All practical workers in metals are aware of the vast difference that the minutest quantity of one metal in excess of the right proportion over another will make in the resulting alloy.

The eagerness of some disputants to avoid the argument of the power of small quantities, as evidenced in vaccination, has led them to assume that cowpox is a microbian disease, and is due to the development of certain microbes in the body. It would have been more to the point if they had proved this instead of assuming it. It is not too late for them to prove it now—if they can.

Mention has been made of the statistics of homœopathy in America. I have before me the figures returned in 1886. There were then thirteen homœopathic medical colleges established under Government authority. In 1886, 1,121 students matriculated at these, and 384 graduated; and in the States there were then 7,345 *alumni* of these colleges. In addition to these graduates of homœopathic colleges, there are numbers who have graduated at allopathic colleges and embraced homœopathy subsequently. The last return gives the total number of homœopathic doctors in the States as 11,000.

JOHN H. CLARKE, M.D.

DR. G. JOHNSON.

[*Jan.* 14.

Lord Grimthorpe's letter contains the following sentence, marked as a quotation: "If they are curable at all, nature will cure them, if we do not manage to prevent it." (G. Johnson and others.)

I know not who the "others" may be, but I beg to say that neither that sentence nor anything like it is to be found in any publication of mine. The clients for whom Lord Grimthorpe holds a brief have evidently supplied him with a mendacious perversion of what I have written on the important subject of the *vis medicatrix naturæ*—a subject which is entirely ignored by those unscientific empirics whose sole aim is the treatment of symptoms by drugs, and who would have their dupes believe that every recovery is a cure.

GEORGE JOHNSON, M.D., F.R.S.

11, *Savile-row, W., January* 13.

MR. ALFRED HEATH.

[*Jan.* 14.

It is difficult to conceive that there should be any educated man so careless of ascertaining the actual facts of the case before rushing into print, or so ready to listen to the gossip of every allopathic chemist's assistant, as your correspondent "R. B. C." His ignorance is almost sublime. His assertions are devoid of a grain of truth or fair argument. I have been excessively amused at his statement as to how our medicines are made, as all your readers will be if they will take the trouble to consult the "Homœopathic Pharmacopœia." They will see at once the blunder into which "R. B. C." has fallen. Every homœopath knows quite well that the pilules and globules are made first from absolutely pure sugar, and they are afterwards medicated with the particular strength of medicine required. If the medicine were added before the sugar was melted it would be spoilt; so much for his knowledge. All his deductions are absolutely beside the mark, and have actually nothing to do with the "law of similars" any more than this, that better cures are made with infinitesimal doses than with crude drugs. There are some medical men who believe that the small dose is the most effectual in curing disease, and they are led to this, not by fancy (all of them having used large doses as allopaths before they became homœopaths), but because their experience has shown that they will act and cure better; otherwise, I presume (as they wish to cure), they would not use them. Others may use larger doses, but no homœopath gives medicine as strong as the allopath does, neither are the homœopathic mother tinctures so proportionately strong as the allopathic tinctures (as "R. B. C." says) when dry substances are used the proportion is generally one in ten. In the allopathic, or British Pharmacopœia, the strength of the tinctures are mostly two and a half to twenty, in some cases one in five. The calculations of "R. B. C." are simply intended to ridicule; there is no argument in them. Perhaps he will be astonished if I tell him that a grain of musk may give its overpowering and characteristic perfume for years and not appreciably lose weight, and that even the imponderable amount of odour perceived will, in some individuals, cause distressing symptoms. I did not intend writing so long a letter; my object in writing at all is to try and show to persons like "R. B. C." that even the 30th dilution will not only act curatively, but it will produce symptoms. Now, Sir, whatever the dilution is, or according to "R. B. C." is not able to do, whether it contain the millionth or the decillionth, whether his absolutely material mind can conceive anything smaller than a milestone or not, I suppose he will grant that a fact is a fact, and that if an infinitesimal dose will produce symptoms, it is evidence that it is strong enough to cure them, especially if given according to the "law of similars." I am prepared to make the following challenge:—If a drug whose effects are well known on the healthy human system be prepared in the 30th potency, I will undertake to tell the name of that drug or some of its most prominent symptoms, although only a letter or a number be marked on the label of the bottle. Of course such a trial as this to convince everyone must be done publicly.

114, *Ebury-street, S.W.* ALFRED HEATH, F.L.S.

The following portion of the correspondence appeared after the hearing of the appeal on January 14, in which the former judgment in favour of Mr. Millican was reversed on technical grounds.

LORD GRIMTHORPE (6).

[*Jan.* 17.

"J. C. B." is determined I shall not pass him over lightly this time, and I will not. But I will dispose of a smaller matter before taking his *verbosa et grandis epistola* in hand. I really cannot see what Dr. George Johnson has to complain of, unless the homœopathic tract "Allopathy judged by its Professors" invented the following utterance for "Dr. Johnson, of King's College," who certainly was and is Dr. G. Johnson:—"The most general and comprehensive statement with regard to the cure of diseases that can safely and confidently be made is this—most of those diseases that are curable by any means are curable by the unaided powers of nature." When doctors talk learnedly to outsiders, as he does now, about the *vis medicatrix naturæ*, they do not mean outsiders to introduce that fatal word "unaided." They keep that for domestic use. I added the word "others" to his name, to save multiplying quotations to the same effect and nearly in the same words, of which there are plenty in the same tract, both as to unaided nature and as to the floundering state of A. physicians in what Sir Thomas Watson called "a sea of doubt on questions of the greatest importance;" and Dr. Todd (both eminent K.C.L. physicians) said that "no department of knowledge demands so urgently the reform of a close and scrutinising induction."

"J. C. B." and all these objectors to experiments which not one of them has ever tried honestly, while they call those who have impostors, have to learn that induction means forming a conclusion from the greatest possible number of instances, and not excluding one that contradicts it; and that a man who refuses to try experiments which are said by credible people to contradict it, and, still worse, who tries to burke them or to suppress any evidence, as the A's have done in sundry places, convicts himself of preferring falsehood to truth. And none the less if he pretends to prove the impossibility of ascertained facts by reasoning which only means that he no more knows their primary cause than why the sun and earth attract each other, nor indeed any more than he knows why any physic or poison generally does what it does. With some we can go a stage further than others in the chain of second causes, but with all we come somewhere to a stop and have to say, "It is their nature to." "J. C. B.," with his usual veracity, is pleased to say that I "give up vaccination," though Dr. Dyce Brown said I was quite right in calling it a strictly homœopathic proceeding, as it plainly is. Only such philosophers as "J. C. B." and "R. B. C." would deny it on the ridiculous ground that we know rather more about why it acts than we do of some others, both A. and H. It is not more certain that vaccination, stings, stinks, and sweet smells, poisoned arrows, poisons so invisible that the chemists who maintain their existence confess that no analysis can find direct evidence of them, in enormous quantities of water often produce great effects, than that homœopathic doses of the proper kind often do what allopathic ones of the best known kinds will

not. Yet they go on writing their nonsense about decillionths, and making no end of mistakes too (if that signifies), as if such writing could alter such facts, or as if the H.'s were any more bound to stick to Hahnemann's decillionths than the A.'s to the black doses and filthy jorums of our youth, which the more honest of them confess that we have to thank H. for reducing. But, again, the doctor of *The Saturday Reviler* (as Mr. Bright well called it), in a fury at being laughed at, which his friends had better look after, has reached a lower logical bathos than even "J. C. B.;" for he considers his former parallel of the truth of gravity and the falseness of homœopathy completely and sufficiently established by the fact that every doctor says so "who is not a homœopathist." I think that staff of superior persons whose wisdom closes the week for us wants a little weeding after such prelections in logic as it has now given us twice. Moreover, that is one of those truisms which are not true. For by no means every A doctor thinks that H is not true, though they dare not say so publicly. The big A's only sit upon the safety-valve to prevent any H. steam from escaping, as it some day will, and blow them off as they deserve.

After "J. C. B.'s" pretence that homœopathic "remedies and diseases" enough to fill more than two pages of a closely-printed H. tract (No. 14) when put together got into the A. "Materia Medica" book all "through the error of a copyist," it is a work of supererogation to expose his credibility further; and yet when one has got a dirty job to do it is as well to sweep it clean. In my last letter I said he might have missed seeing the paragraph correcting my self-evident error of "England" for "the world" the day before any answer to it appeared, but that he could not have overlooked the correct version in my next letter. That would have been enough for most people, but it is not for him. I must be a liar because I defend the homœopathists, I suppose, for he gives no other reason except this convincing one, to be sure, that I also quoted from a tract of earlier date that there were then 10,000 H. doctors in the world, and then, from information sent to me the other day, that there are now 11,000 in America alone, which are perfectly consistent for different dates. That sort of thing, he says, may do very well for a "slip-shod scientist like Mr. Proctor," but will not go down with a genuine philosopher like him. I am glad to see him expose his own well-shod science and knowledge by applying such epithets to an astronomer who is known all over the world as having corrected and advanced on some of the most celebrated ones in various matters. As he likes legal analogies, he reminds me of what a northern client of mine once said behind me when I was cross-examining a sort of "J. C. B."—"My eye, but that chap's a lunger." He had better mind his own business and try to answer the charges against his own fraternity, of which I keep receiving more and more evidence after every one of these letters.

Did you ever read anything more contemptible than his answer to that case of the "regular" who had pronounced a lady dying of cancer within a month, and refused to examine her again, because her father, whose name I said would command attention, had meanwhile consulted a H., who had presumed to doubt this regular diagnosis, and, worse still, turned out to be right—a little fact which

he naturally drops. My statement of the case was so "lame" that he can make nothing of it, which is very true; "if the lady lived where no other medical assistance could be got (which he must know from my description was impossible), then he should say the regular was wrong;" if not, that he was right, as he had been "discarded" (which he had not), and he could not carry out homœopathic treatment, which he was not asked to do, unless looking at a patient who has been looked at by a H, is carrying out homœopathic treatment." We know it is heresy already. Then he runs off into another guess at legal practice, and mine in particular, which is as wrong as all the others of himself and his colleagues.

Perhaps he would like to try his hand on another case or two of "medical ethics," as these people call it. Here are two then, just come from an old friend whom I had not heard of for I do not know how long, since I was told he was dangerously ill. And so he was, and had been drenched by all the best "regulars" of the day, and finally told that he had a disease which would finish him in six months, as it always did if it could not be cured speedily. He did discard them after they had so discarded him, and went to a H, by whom he was straightway cured, and lives, like Wycliffe, "to expose the evil deeds of the (allopathic) friars." Here is another of them, a still nicer case for "J. C. B.'s" casuistry to dispose of, from the same friend. A formerly well-known, very rich man was pronounced so very near dying that the eminent "regular," who was doing his best to aid or to beat nature, told his sister that she might give him anything she liked; and just as a matter of civility, or business, called the next day and inquired at what hour he died. "Died, sir," answered the footman, "he's had a very good night, and is much better." Of course he went in and asked what had she given him? "A homœopathist." As soon as he heard that, he got into such a passion that she had to tell the servants to turn him out. She might have helped her brother off with too large a dose of laudanum by mistake, as a great London doctor sent for "special" was considered to have done to a well-known Yorkshireman, or by any other too big dose; but helping him to stay in the world by a little one (as he did for years after), was much worse than manslaughter.

If that too was a case on which "J. C. B." would like to reserve judgment till I can tell him some immaterial facts about it, here is another from a quite unquestionable source, with all particulars—unless I have been imposed upon by a correspondent, who writes to me exactly this:—

"From a pompous (I should have added catch-penny titled) book, 'The Stomach and its Difficulties,' by Sir James Eyre, M.D., &c., ninth thousand, 1879, p. 63:—I look back with much satisfaction upon an instance, where (being a medical director, that is, physician, for there was no other examiner during eight years, at an insurance office) I refused the life of a nobleman of high rank, *because* [italics in book] his medical man was a H, and my brother directors concurred with me unanimously."

That is, in plain English, this medical director and sole examiner looked back with much satisfaction on having cheated the company, who paid him for only telling them the truth about the value of

insurers' lives, and having done his best, out of mere spite and ignorance, to deprive a family of some thousands of pounds because their father had consulted a heretic. No doubt, however, that was avoided by the nobleman going to some other Board less cowardly with some other doctor less dishonest or idiotic. He could not possibly have believed that the nobleman's constitution was secretly undermined by homœopathic medicines, because he believed, or would have sworn he did, that they do nothing. And yet he solemnly rejoiced that he made the company reject a life, otherwise good, solely because the man only took innocent medicines. Perhaps by this time he rejoices less.

It is worth while to add, as a practical commentary on that disgraceful confession, that it appears in the discussion in *The Times* about Lord Beaconsfield's treatment, in 1881, that in New York there actually is a company which insures homœopathic lives for 10 per cent. below the ordinary premiums, and is well justified by the result; for in the latest period given, only one in 75 of such lives had died against one in 30 victims of allopathy, out of about 11,000 lives altogether, of which the H were the most numerous; again a superiority of more than two to one, as in the hospitals. One such fact as that—and nobody in the Beaconsfield correspondence disputed it—is worth 1,000 columns of argument to prove that such facts have no business to exist. Even Sir Joseph Banks, P.R.S., had to confess at last (see Peter Pindar) that "Fleas are *not* lobsters, d—— their souls," though he had proved beforehand that they were.

Again, one such fact, though a more private one, as this, among those which have been sent to me, is worth more than any amount of argument; for there can be no mistake about such cases. A clergyman to whom money was of the usual consequence was told by the surgeon of a special hospital, and by another of St. Bartholomew's, of well-known reputation, that he must have a serious operation, which would cost him £50. By chance he was advised to consult a provincial H first. He did so, and was cured at the cost of 30s., ten years ago, and has been quite well since. I cannot remember a single instance where any A doctor has done the smallest atom of good to me or any of my relations, friends, or servants in the commonest of all semi-chronic ailments now, rheumatism, though I have in rheumatic fever, more or less. The alternatives are—(1) "Unaided Nature," whom I am sorry to say I must leave as she has always left me, on a level with the A's. (2) Bath and Buxton and some German baths. I have often been to the two former, and am going again to the first in a day or two, because I have never found them fail for myself, and very seldom for my friends and hospital patients—perhaps once in ten times. (3) Homœopathic bryonia, of which I have found so little as a single globule a day do so much good that I should not think it worth while to go away now, except that I find it generally beneficial for the year. And I have this further remark to make about those waters. As "J. C. B." knows all about vaccination, and poisoned arrows, and stings and stinks, and everything, except H medicines, can he tell us why such waters cure rheumatism, and especially the Buxton ones, which are the freest (one cannot put in three e's) from mineral ingredients, and the coolest, and yet much the

strongest, as measured by the time you must stay in? If he can, *erit mihi magnus Apollo*, for nobody at Buxton can; they are wisely content with experience and success, and so am I; and I prefer them infinitely to theory and failure. Sometimes of course everything fails, as the A's do always with rheumatism.

He asks, with an affectation of indignation, what I mean by saying that he and his fellows have not even attempted to answer that array of their confessions of incompetence and confusion of their so-called scientific medicine, when I must know that it was only made up of "scraps riven from a context which would have put a different complexion on them." Well, if I must tell him what I mean, it is that he knows very well that there is not a single word of truth in that assertion; first, because from the nature of those quotations, which nobody disputes are accurate, it is impossible, and, secondly, because if it were possible it is quite certain that he or somebody else would have shown it fast enough. I gave them the full reference to the tract, and probably they knew it well enough before. And therefore I now sum up their case by saying that they have given no rational answer to a single one of the charges that have kept pouring in against them since they pulled the string of the shower-bath some weeks ago, of these domestic confessions of their impotence and ignorance; of persecuting everybody that they can or dare who takes them at their word, and practises or encourages something else; of tyranny over hospital committees; of deceiving and boasting that they had deceived insurance companies who paid them to tell the truth; of preferring that their patients should die in the odour of allopathic sanctity rather than be cured by heretical small doses; of suppressing in one case here, and embezzling in another at Vienna, returns which were against them; of charging with ignorance and "intellectual barrenness" those who necessarily know both systems, while they know only one. I hope they like this summary, and are duly grateful to "J. C. B." for inviting it.

I do not suppose anybody cares, except for curiosity, whether there is any more than that initial identity which Dr. Dudgeon noticed between "J. C. B." and Dr. J. C. Bucknill, who tried in vain to persuade the College of Physicians to denounce homœopathic doctors *ex cathedrâ* though they are bound by Act of Parliament to license them as much as themselves, and prohibited from revoking any such licence afterwards for that reason. One "J. C. B." may be ashamed of the other for what I know. I merely take this one as the impersonal representative of allopathic wisdom. But if anybody else cares for his apparent denial, I warn them that "J. C. B." might swear the final sentence of his letter as an affidavit, and yet be clear of perjury, if he vanished like "Dr. Jekyll" and J. C. Bucknill suddenly appeared in his shoes.

The Court of Appeal has decided that the Jubilee Hospital Committee had a legal right to turn out Mr. Millican for associating with homœopathists, or for anything else. And it was clearly only a question of time, and whether they had done it regularly. The moral question remains exactly where it was, and it is now clearer than ever that some more of either legal protection or enlightenment of the public is necessary. Committees are mere tools of the majority of

doctors, and I have always steadily refused to be on any hospital committee for that reason. Subscribers are as much sheep as shareholders generally are. It is time that either Parliament, or some resolute Minister with control over some national hospitals, should insist on fair play being given to homœopathists and homœopathy by a sufficient period of fair experiments, not sham ones, and unquestionable, and undoctored, and even un-nursed reports, for they will have all that to contend with. All public hospitals should be obliged to report to the public truly.

I hope everybody read the admirable letter of the "Sceptic" on January 12, which I suppose not two men in England could have written, notwithstanding "J. C. B.'s" impudent affectation of consigning it to "a medical journal." Of course he knows the value of such a limbo—especially as it seems the A journals refuse to print even advertisements of anything homœopathic. Now I think I have done enough for both sets of doctors. The A's pretend to laugh at it abroad, but I know what they are saying at home.

GRIMTHORPE.

DR. MILLICAN (5).

[*Jan.* 17.

I am not prepared to accept "J. C. B.'s" and "R. B. C.'s" contention that smell is produced by waves comparable to those of sound and light, which produce the senses of hearing and sight, in the face of Dr. Michael Foster's view that it is the result of the contact of material particles.

As I am unable to see the bearing of Dr. Dupré's argument, I can form no opinion as to its value.

But these do not affect my position in the least degree. I am prepared to discard them on disproof, without finding it requisite to change front with reference to the extension of professional fellowship to homœopaths.

"J. C. B." speaks of me as "making rapid progress" in my "homœopathic education." Upon what grounds does he make this assertion? Merely because I adduce three well-known circumstances apparently as incomprehensible as the infinitesimal dose theory, and tending to illustrate (unless and until the analogy be proved insufficient) the bare possibility thereof.

It is just such thoughtless misrepresentation—if thoughtless it can, indeed, be called, judging from "J. C. B.'s" first letter, though I am willing to give him the benefit of the doubt—which envenoms controversy of all kinds. Surely a man owes it to his own self-respect either to master his adversary's contentions and try to construe them fairly, or if he considers him entirely impracticable to say so and forbear argument altogether.

A little cosmopolitanism in acquaintance—which the older ethics of the profession have not encouraged—makes earnest men less dogmatic in their own opinions and more inclined to do justice to those of others.

KENNETH MILLICAN.

A SCEPTIC.

[*Jan.* 17.

You have placed two and a-half columns at the disposal of the voluminous "J. C. B." and the nonluminous "R. B. C." You could not possibly have made a better use of the space. Nothing can more powerfully further the spread of rational and healthy scepticism than the letters of these two dogmatical gentlemen.

"J. C. B." informs us that "stinks are due to odorous particles or gases coming in contact with the mucous membrane of the nose and stimulating the nerve-cells there." In the next column but one "R. B. C." tells us that "there is no evidence that the sense of smell is excited by detached particles; what evidence there is and all analogy point to an opposite conclusion." There you are! Two luminaries of medicine, each cocksure about everything, and each brimful of contempt for everybody who does not bow humbly before their infallibility, contradict one another flatly and categorically upon a point which lies at the very threshold of "medical science." The smell of musk gives some people a headache, the smell of roses will do as much for others, the smell of a man a mile off will put a herd of deer in motion, yet "medical science" has not the ghost of an idea how smell comes about. This is a comparatively simple and external physiological affair, yet we are asked to believe that men who know nothing about it are competent to say with certainty what goes on in the inmost recesses of the organism when a grain or a decillionth of a grain of a drug is administered.

Neither of your correspondents has so much as got the scientific habit of mind. "J. C. B.'s" habit is wholly forensic. He is the Sir William Harcourt of this controversy. Like that eminent man he busies himself with declamation about the fringes and accidents of the discussion, never by any chance settling down to close and dispassionate examination of the real question. Also like that eminent man, he lays about him in a manner which his antagonists themselves must admit to be amusing, but when his fireworks are over the argument is found not to be advanced by a single step.

"R. B. C." is forensic too, and fires off plenty of crackers, but he has a comical consciousness that he ought to be scientific if he only knew how. Just observe how completely he is enslaved by vulgar notions of magnitude, which it is the first business of the physicist to escape from. Had the milligramme instead of the grain been the English unit of drug measurement, people like "R. B. C." would have accepted the first dilution of the homœopaths as quite in the order of nature. He never saw a decillionth, and cannot conceive it; therefore he is cocksure there can be no such thing. Before the microscope was invented he could not have seen or conceived a blood corpuscle or a spermatozoon; but if there is one thing that the microscope teaches more clearly than another it is that its limits correspond to no real boundary in nature, but only measure the imperfection of the human observer. While drawing hard-and-fast lines concerning magnitudes on one side, he has no faculty for appreciating them on the other. He based an argument on the number of cubic feet in the terrestrial globe. He stated that number at just about one ten billionth of its true magnitude, and when the

mistake is pointed out he says it does not matter. Unity and ten billions are all the same to "R. B. C." when it suits him; but when a homœopath uses a billionth instead of "R. B. C.'s" grain he is a fool. Observe, again, the confusion of "R. B. C.'s" ideas about matter. He says smell is not produced by the impact of particles upon the olfactory nerves, but by movement. Movement in what? Movement of what? "Air or ether," says "R. B. C." with the fine *insouciance* of a man not particular to twelve places of figures, and not pedantic about historical or biographical accuracy. Air or ether; you pay your money and take your choice, just as with Ringer's cures for constipation. Air we know something about, we can feel it, breathe it, and weigh it. Ether is the mere postulate of a theory. We know nothing about it except that if you grant its existence and do not prove too exacting in the matter of definition of its qualities, then a working hypothesis concerning the nature of light may be constructed. Take it as they vaguely conceive it, and you will find that an ounce bottle full of air bears the same unthinkable relation of ponderability to a solar system full of ether that a material dose does to a decillionth. Still, air or ether is all one to "R. B. C."; but you must accept his dictum about movement without question else you are a fool, and at the same time you must believe with "J. C. B." that it is not movement, but particles, else you are a knave.

"J. C. B." does not want to discuss with me the actions of drugs. Convenient blindness! What I discussed was not the action of drugs, but the ignorance of their action and the contradictory jumble of ideas that we are asked to accept as "medical science." He says I forget everything except therapeutics, whereas the larger part of my letter dealt with what they call pathology. But a chain is only as strong as its weakest link, and all the pathology and vital chemistry in the world will not cure a man unless it can indicate the proper treatment. Anybody who has hit upon the proper treatment, no matter how, will cure him without knowing as much pathology as would suffice to fill a paragraph with unproved assertions.

I should write a longer letter than "J. C. B." himself were I to expose all the contradictions and absurdities of the orthodox champions. But I have no "pathy" and no sect to defend, no interests to be fought for, and no temper to be roused. If the homœopaths had the whip-hand it would very likely become a duty to rebel against their dogmatism and intolerance. At present it is the other gentlemen who sit upon us, and to point out the slenderness of their claim and the monstrous arrogance of their attitude is all that concerns,

A SCEPTIC.

DR. T. L. BRUNTON.

[*Jan.* 17.

I thought that in the preface to the third edition of my work on Pharmacology, Therapeutic, and Materia Medica, I had expressed my opinions regarding homœopathy sufficiently clearly to prevent them from being misunderstood; but as the writer of a letter in *The Times* of the 12th inst. says it is difficult to come to any other conclusion than that I am half a convert to homœopathy, or at least regard it with favour, I should like to correct this misapprehension. My

opinions are that what is true in homœopathy is not new, and what is new is not true, or, as I have expressed them in my preface, "In founding the system of homœopathy, Hahnemann has proceeded with his facts as he did with his medicines, diluting his active drugs with inert matter, and diluting his facts with much nonsense." Unless for the purpose of correcting a mistake regarding my personal opinions, I should not have troubled you with this letter, nor shall I occupy your space with any attempt to defend my opinions, for I do not intend to take any part in the controversy now going on. Those who desire the reasons I have for my opinions will find them either in the preface to the third edition of my work on Pharmacology, Therapeutics, and Materia Medica, or in the *Monthly Homœopathic Review* for June, 1887, where my preface is re-printed and adversely reviewed. This review is re-printed in a separate form as No. 15 of the "Tracts of the Homœopathic League." For the benefit of those who may take the trouble to read the review, I may mention that my authority for saying that Hahnemann suffered from ague, of which, according to the reviewer, there is no evidence, is to be found at page 104 of Ameke's "History of Homœopathy," translated by A. E. Drysdale, M.B., and published for the British Homœopathic Society by E. Gould and Son.

T. LAUDER BRUNTON.

10, *Stratford-place, W.*

DR. J. C. BUCKNILL.

[*Jan.* 17.

I had hoped to have kept myself quite outside of this "Odium Grimthorpe." When I see hay on the horn I am glad to get behind a tree, but the reference to me by name in your columns of the 12th inst. by your correspondent Dr. Dudgeon made it imperative upon me that I should write to you to disclaim having had any hand or part in the letters you have published under my initials, "J. C. B." I do not even know who is the author of those letters. If Dr. Dudgeon had taken more pains with his diagnosis he would possibly have found rational grounds for the opinion that the letters were not written by me; but in true homœopathic fashion he allowed himself to be convinced by the most prominent symptom—namely, the signature, and thus jumped to a wrong conclusion, which he, no doubt, will now regret.

Dr. Dudgeon, moreover, smarting under the lash of the other "J. C. B.," whoever he may be, charges me with having made accusation of dishonesty against colleagues as well educated and probably as honest as myself, and in support of this charge he quotes a "motion proposed" by me to the College of Physicians in 1881. But Dr. Dudgeon will perhaps be a little surprised to learn that I do not consider that I have ever made any accusations of dishonesty against homœopathic colleagues. I have but suggested dishonesty as one of the horns of a dilemma of which incompetence was the most prominent; and surely it must be admitted that in medicine, politics, theology, and other phases of ideational authority in which some men profess to guide the lives of others, the honesty of the agent in relation to his knowledge must be considered. But beyond these generalities, I think I may fairly ask your permission to explain the particulars

under which the offending motion was made. At the date mentioned the President of the College of Physicians had summoned an extraordinary comitia to pass some sort of judgment on the homœopaths, and at this comitia the following resolution was duly proposed :—

"While the College has no desire to fetter the opinions of its members in reference to any theories they may see fit to adopt in the practice of medicine, it nevertheless thinks it desirable to express its opinion that the assumption or acceptance by members of the profession of designations implying the adoption of special modes of treatment is opposed to those principles of the freedom and dignity of the profession which should govern the relations of its members to each other and to the public. The College, therefore, expects that all its Fellows, members, and licentiates will uphold these principles by discountenancing those who trade upon such designations."

To wit, the homœopaths. When I heard this I could not forget that the College of Physicians itself did not discountenance those of its Fellows who traded, not in designations, but in patients who were incarcerated in their private lunatic asylums, and I had the audacity to move an amendment in the terms quoted by your correspondent—namely: "No competent medical man can honestly practise the so-called homœopathic system."

This was brief; I think it was intelligible, and I am sure it was not intended to be insulting, and if Dr. Dudgeon will take the trouble to turn it over in his mind with impartial criticism I shall not be surprised if he admits it to be a not unfair statement of the allopathic contention. If its converse be true, then *cadit quæstio*, and allopathy is a vain delusion; for it is not possible that both allopathy and homœopathy can be practised in good faith by competent physicians. I have read the letter of your able correspondent "Sceptic" with great interest, and yet I do believe in orthodox medicine and in the justice of its claim to be considered a science and one of the highest, if not the very highest of all in importance. No doubt it is an immature science, and from the vast extent of its relations it will probably always remain so. But it is built upon the sure foundations of biology and chemistry, from which henceforth it can never be shaken. Its professors, comparatively ignorant as "Sceptic" thinks them, are yet diligent seekers for brighter light and better knowledge, and by aid of the scientific methods they now use they are quite sure of their attainment.

But almost the converse of all this may be said of homœopathy. Its facts are mostly accidental observations, unconnected with the organised systems of knowledge, and therefore, even if true, destined to be unprolific. Its principles—if principles they can be called—are dogmata, inspired by the very spirit of paradox. Take the dogma of the infinitesimal dose. Is there anything in nature, the homœopathic drug alone excepted, the energy of which increases as the substance decreases? Is not this dogma as if some new school of mystical physicists should declare that gravitation decreases with the substance and increases with the distance of gravitating bodies? Is it not, in truth, *contra naturam*, and, in the absence of miraculous intervention, impossible and incredible?

Then, as to the second dogma that, *similia similibus curantur*, does

all nature contain any other instance in which motion of any kind is checked or stopped by similar motion in the same direction? In the magnificent experiment of breeding in men, animals, and plants, which nature is ever carrying on for our instruction and delight, and by which we can ourselves interrogate nature in the most subtle manner, do we find that diseases and defects in progeny can be avoided in new progeny by parents having the like diseases and defects? Or is not the contrary true? In the world of mind, do we quench passions by the motions of like passions? Does "Sceptic" try to cure our conceit, as he thinks it, by the administration of faint praise, or by heroic doses of contempt and censure? No; this dogma also is pure paradox, whose only home is in the mystic mind.

The homœopaths in their league pamphlets call me "mad-doctor," and under that designation they must allow me to tell them that I take a deeper interest in their minds than in their medicines, and that I have been greatly perplexed to find a rational explanation of the fact that men of such high intelligence and culture, as some of them undoubtedly are, can steadfastly believe in such paradoxes as the above, and can use them as principles of action wherein the lives of multitudes of men stand in peril. The best explanation I can afford is the hereditary transmission of tendency to mystical thought, which is not and cannot be confined to what are called spiritual matters. In the duration of the human race the date of the "Novum Organon" is as of yesterday, whereas the cerebral tendency to mystic notions has been inherited through unrecorded æons; and perhaps the closest congeners of the homœopaths may be the most remote, and have existed in those practitioners of the Stone period who trephined the skulls of the cave-men with the supposed intention of letting out the spirit of disease.

JOHN CHARLES BUCKNILL.

Hillmorton-hall, Rugby.

LORD GRIMTHORPE (7).

[*Jan.* 18.

After correcting a misprint which appeared in his previous letter, his lordship continued:—

Dr. Bucknill has not improved the allopathic case by showing that the vapid resolution of the College hits mad-doctors, hydropathic doctors, throat doctors, oculists, aurists, dentists, not one of whom they have ever dared to meddle with, quite as much as homœopathists. The other "J. C. B." could not have beaten that. I hope that they have all seen that a lot of Irishmen were condemned to five weeks in prison for "boycotting," as conspiracy, and the conviction was confirmed the other day.

GRIMTHORPE.

DR. R. E. DUDGEON (3).

[*Jan.* 19.

At last the great original anti-homœopathic champion, as he informs us, is going to retire from the controversy. Possibly he "sheaths his sword for lack of argument," but before doing so he discharges a Parthian dart, but as it is aimed chiefly at Professor Foster we may pass it by, and leave the professor to defend his statement about smell

being excited by detached particles from an odorous body. Possibly most people will incline to attach more value to the opinion of the "eminent physiologist" that to that of his critic. "R. B. C." now seems to think that it does not affect his argument whether the earth contains 250,000 million cubic miles or only 4,000 million cubic feet. He takes his calculations, as he tells us, from Simpson's "Tenets and Tendencies," and apparently his knowledge of homœopathy, as well as that of "J. B. C.," to judge from an admission in his letter, is derived from the same pure source, which is about as appropriate a work in which to study homœopathy, as would be the writings of the late Thomas Paine for the study of theology, or the Comic Blackstone for the study of the law. "R. B. C." must have another fling at the itch-insect before he goes. He now insinuates that Wichmann's drawing of the itch-mite was of some other insect not uncommonly seen on dirty patients—possibly Burns's

> "Ugly, creepin', blastit wonner,
> "Detested, shunn'd by saunt an' sinner."

Unfortunately for this guess, Küchenmeister (vol. ii., p. 23), who is, or was, the greatest authority on these matters, tells us that Wichmann knew all about the itch-mite. As my chief business in this controversy has been to correct the errors of "R. B. C.," I rather regret that he now "throws up the sponge," for I think that if he had only gone on a little longer, he might have eventually come to have some real knowledge of homœopathy slightly different from what he has learnt from Simpson.

"J. C. B." at all events shows no signs of exhaustion, for he gave us on Saturday the longest letter of the whole series, but I hardly think its lucidity is proportionate to its length, indeed, it seems rather to tend to darken counsel. "J. C. B." is something like the mollusc from which we derive one of our medicines, sepia, the more ink he sheds the more obscure does it become all around him. He ("J. C. B.," not the cuttle-fish) says two grains of strychnine and one-eighth of a grain of digitaline will infallibly kill a man, and these doses "are a whole universe greater than a decillionth of a grain." It may be so, but then we do not want to kill a man, but cure him, which makes all the difference.

It strikes me that "J. C. B." might find more analogues for Mrs. Stephen's specific in the prescriptions of his own school than in those of ours, for one of our maxims is to give medicines singly and alone, and not to mix up a quantity of heterogeneous substances in one prescription, as is done by Mrs. Stephen and our allopathic friends.

It may be true that in this country only 258 medical men have adopted homœopathy, while 22,669 have not adopted it, but then it should be remembered that these 258 have carefully studied and tested it, while the 22,669 have neither tested nor studied it. If this is an argument against homœopathy, it strikes me that the Irish criminal who offered to bring twenty witnesses to prove that they did not see him commit the crime to upset the evidence of two witnesses who swore they saw him do it, was wrongfully condemned. If he had had a jury of "J. C. B.'s" he would certainly have been acquitted.

Doctors, says "J. C. B.," have examined the proofs of homœopathy "again and again, and found them wanting." I know a good deal

about the literature for and against homœopathy, and have not yet met a single instance of an intelligent examination by doctors of the proofs of homœopathy in the only possible way—viz., by careful trial at the bedside—that was not followed by entire or (in one case, that of Kopp) partial acknowledgment of the truth of the homœopathic rule. As "J. C. B." knows that this has been done with an opposite result "again and again," I call upon him to refer me to any published record of any such examination. "J. C. B." dislikes statistics, and no wonder, they are all against him; in this he resembles the French theorist, who declared "If the facts are against my theory—so much the worse for the facts." But "J. C. B." could make "mince-meat" of our statistics if he only had space. Well, I daresay any of the medical periodicals of the old school will willingly give him as much space as he requires, for there is nothing they so earnestly desire as a "destructive analysis of these figures." We, too, should be charmed to witness his attempt. We do not much care for what Professor Gairdner "maintains," as from experience we have a very poor opinion of his "candour and open-mindedness" in the matter of homœopathy.

The number of homœopathic practitioners in the world varies from year to year. The pamphlet from which Lord Grimthorpe quoted was published in 1881, at which time there were about 10,000 medical men practising homœopathy in the world. But as from 300 to 400 are added every year to the number in the United States it happens that the total number there at last year's enumeration amounted to about 11,000. In all Europe there are about 1,000, and probably there are upwards of 100 in other parts of the world, which make the total number of practitioners of homœopathy about 12,100. The reason why homœopathic practitioners are so much more numerous in America that in Europe is that there being no monopoly of medical education by one sect in the New World, they have plenty of colleges and even a University where all the professors are favourable to homœopathy; whereas in the Old World all the medical schools are in the hands of the opponents of homœopathy, only one University, that of Budapest, having a chair of homœopathy; but, as the study is not compulsory, the Hungarian students do not attend the professor's class, which is frequented only by those who have taken their degrees, or by foreign inquirers.

"J. C. B.," with the magnanimous condescension of the superior person he undoubtedly is, says:—"I have not attributed dishonesty to any individual homœopath; I have always maintained that there are many warp-brained or weak-minded members of the sect." Well, I suppose we ought to feel grateful at being assured on such high authority that we are all either knaves or fools. Possibly it is better to be a knave than a fool, for there are hopes that a knave may reform, but for a fool there is no hope—"Er bleibt ein Narr sein Leben lang."

"J. C. B." falls foul of my poor little instrument, the pocket sphygmograph, which may be "an ill-favoured thing" likely enough in his eyes, but is "mine own," and so, though I never would have dreamt of bringing it into the controversy, I feel disposed to defend it now that "J. C. B." has drawn it like a red herring across the scent.

"Why," he says, "one might suppose that Dr. Dudgeon, and not Vierordt, had invented the instrument, while all he has done has been to make some infinitesimal mechanical alteration on Pond's American sphygmograph, thus producing an instrument which Dr. Burdon Sanderson and Mr. Hawksley (and where shall we find higher authorities ?) regard as most untrustworthy." Now, if "J. C. B." will do me the honour to read my little book on the spygmograph, he will find I have rendered all honour to Vierordt, to whose instrument mine has no more resemblance than has a "hawk to a hand-saw." Dr. B. Sanderson in 1867 (about fourteen years before my sphygmograph came out) published a "Handbook of the Sphygmograph," in which he extols Marey's instrument altered by himself, but I was not aware that he regarded mine as untrustworthy, and I am sorry to hear it. Mr. Hawksley is a very respectable surgical instrument maker, who, as he himself told me, was much employed by physicians in taking sphygmograms with the cumbrous and difficult instrument in general use before mine, which required the skill of an expert to apply properly. He was therefore naturally not biased in favour of my instrument, which is so simple as to require no special skill in its application, and may be used by a child. I did not know he was a high authority in the science of sphygmography ; but we live and learn. In this connection I may mention that I have before me a letter, dated April 16, 1882, from the late Dr. Mahomed, the inventor of a modified Marey's sphygmograph, which was the favourite before mine appeared. Dr. Mahomed writes :—"I constantly use your little instrument, which I find most convenient for ordinary work." As many thousands of my instruments are in constant use, not only in this country, but in Germany, France, Italy, and America, and as it has almost everywhere superseded the instruments formerly in use, I may dispense with the approbation of Dr. Sanderson and Mr. Hawksley. I am sorry to have been obliged to dwell so long on this subject, but "J. C. B.'s" attack on my instrument, though it cannot injure me, as I have never had any pecuniary interest whatever in its sale, touches my vanity as an inventor.

I pointed out the remarkable coincidence that the initials of your correspondent "J. C. B." were the same as those of Dr. J. C. Bucknill, who had also distinguished himself by charging his homœopathic colleagues with dishonesty in the motion he endeavoured to get the College of Physicians to pass. He now denies that he ever brought a charge of dishonesty against us ; his motion only implies that we are either incompetent or dishonest—fools or knaves, in short—the usual allopathic argument. He now tells us that he proposed this motion because the College had not discountenanced "those of its Fellows who traded in patients who were incarcerated in their private lunatic asylums." The logic of this proceeding would hardly be appreciated outside the walls of a lunatic asylum. The resemblance between "J. C. B." and Dr. J. C. Bucknill extends also to the amount of their knowledge of homœopathy. Both might with advantage attend to the admonition conveyed in the heading of one of the chapters in "Tom Jones," "an essay to prove that an author will write the better for having some knowledge of the subject on which he writes." Dr. Bucknill will perhaps be surprised to learn

that we have no "dogma" of the "infinitesimal dose." Doses are a matter of experience, and homœopathy may be and often is practised without the use of so-called infinitesimals. Neither is there a dogma *similia similibus curantur*. Hahnemann's formula is *similia similibus curentur*, "let likes be treated by likes," which is a therapeutic rule deduced from observed facts and not a dogma at all.

So there is little to choose between "J. C. B." (the other claimant for these initials being Sir J. C. Browne) and Dr. J. C. Bucknill. Like the two Dromios, they resemble one another in many points. *Arcades ambo!* Both are "mad-doctors." Both speak dogmatically on a subject they know little or nothing about; both insult their colleagues by leaving them the choice between being considered knaves or fools. To both I might address the words of an old controversialist—"No doubt you are the people, and wisdom shall die with you." My modesty will not allow me to continue the quotation.

In the meantime I challenge those who are always prating about the scientific character of old-school medicine and the great principles of therapeutics to mention one single principle, one single guiding rule of anything like general applicability to be found in their school. They know they cannot; the homœopathic rule still holds the field, there is no other general therapeutic rule or principle known to medical science.

One word of advice I would venture to give to my opponents—If your cause is a bad one, it cannot be made a good one by accusing your opponents of dishonesty and mendacity, and affecting to treat them with lofty scorn. If it is a good one, it can only be injured by such devices. "La promptitude à croire le mal sans l'avoir assez examiné est un effet de l'orgueil et de la paresse."

As regards Dr. Brunton's disclaimer that he is "half a convert to homœopathy," considering his wholesale depredations from the homœopathic *materia medica*, I will leave your calculating correspondent "R. B. C." to find out what fraction of a convert he is. If, as he says, what is new to homœopathy is not true, then the new remedies and new uses of old remedies he has taken from homœopathy, at first or second hand, it matters not which, stamp his "Index of Diseases and Remedies," of which they form such a large part, with the character of untruthfulness; a pleasant reflection for those who take his work for what it claims to be in its title, a "text-book." Dr. Brunton does not certainly act a friendly part towards the homœopathy from which he so largely borrows. It is not so long since he refused to insert the advertisement of the work on the "History of Homœopathy," which he mentions in his letter, in the periodical he edits, *The Practitioner*. Perhaps it is remorse for his previous unkindness which leads him now to give this advertisement of the book in your pages, which is worth more than a decade of continuous advertisement in his own journal.

Mr. Dupré's letter commences so insolently that it scarcely deserves notice. As regards his chemical experiments, it would have been more to the purpose if he had himself made the dilutions according to Hahnemann's method and analysed them in comparison with the preparations he got from the druggist. Until he does this, I shall quite believe that the preparations were what they were represented to be, and yet that Mr. Dupré's analyses were as negative as he states.

R. E. DUDGEON.

Mr. E. G. SWANN.*

[*Jan.* 19.

I am afraid it is too true that, on strict investigation, the statement that large quantities of globules, pilules, and tinctures, supposed to be medicated, are sold just in the condition in which they come from the confectioners and distillers—except, as to the tinctures, that a considerable volume of water is added—must inevitably be proved to demonstration. I doubt if one hundredth part of the ostensible medicines could escape such a result of inquiry—speaking of the past, of course. I feel certain none at all have hitherto ever been prepared for trade purposes in this country in accordance with the Homœopathic Pharmacopœia, on the compilation of which I take my stand. Some of those who profess to prepare and dispense these medicines now, I see, disclose the fact that they do not even know how they should be made. One says that the tincture, attenuated to the proportion of the decillionth of a minim, would only require 12½ tablespoonfuls (I suppose he means 12½ tablespoonsful, or somewhat over six ounces) of spirit in all. Well, for purposes of stock on sale, such things as aconite, nux vomica, and 30 or 40 others, at any rate, would have to be prepared by, at the very least, the pound of globules or pilules, or by the pint of diluted tincture; and it would be impossible to properly steep one pound of globules or pilules in less than one pint of tincture, for each attenuation wanted; so that for the preparation of every single pound or pint of every attenuation or dilution truly prepared, very many gallons of spirit of wine must have been used. I need not bother you with rows of figures, because the general suggestion is enough. I would also remind those who evidently profess to be homœopathic "chemists" that the saturation of globules or pilules in prepared tinctures of dilute spirit would not do, because they at least become sticky if they do not deliquesce; and, further, that supposing a pint of tincture at the 30th dilution be used—that is, a pint of rectified spirit of wine supposed to contain one decillionth of a minim of this or that medicine—it would be impossible to fix the quantity absorbed (if any were absorbed) by each separate globule out of a pound or more. I speak, of course, about the preparations as they profess to be made by "homœopathic chemists," not as they ought to be made. But that, in truth, very few indeed, if any, have been made at all, even as they have professed to be made, I grieve to declare as my conviction, derived from a very close and very melancholy insight with technical knowledge.

EDWARD GIBBON SWANN, author of "Laurie's Homœopathic Domestic Medicine Great Edition of 1850."

---

* Answers to this letter will be found in the Appendix. Mr. Frederick Ross (of Leath & Ross, publishers of *Laurie's Domestic Medicine*) wrote a letter to *The Times*, showing that so far from Mr. Swann being the "author" of the work referred to, he was merely employed as a literary person to collect some of the material, but all had to pass under Dr. Laurie's judgment before it was received, and to Mr. Swann's great annoyance the greater part of it was rejected.

J. C. B. (3).

[*Jan.* 20.

Lord Grimthorpe can scarcely expect me to go on giving him lessons in elementary biology, more especially as the instructions already bestowed on him seem to have fallen on stony ground. I will, therefore, merely point out that he has not yet grasped the difference between a living organism and an infinitesimal particle of inorganic matter. There may be in water, as he says, disease-causing elements, micro-organisms, which the chemist cannot discover, any more than he can discover a decillionth in a homœopathic globule; but the existence of these micro-organisms is proved in other ways, by the uniform effects of the water containing them upon large numbers of persons consuming it, by microscopic examination, and by artificial cultivations, whereas the decillionth cannot be recognised by its effects on human beings or animals or by any other method of investigation—for the homœopaths had to give up their sham magnetoscope long ago; and, again, the micro-organisms in water have the power of increasing its population at a stupendous rate, whereas the decillionth must for ever remain a hermit in its solitary cell. A decillionth (and I am now answering "A Sceptic" as well as Lord Grimthorpe), although inconceivably small, may exist like any other infinitesimal division of matter, and until ultimate atoms and molecules have been measured (if they exist) we cannot dogmatise upon the subject; but what doctors assert is that the decillionth is practically non-existent, that not one shred of scientific evidence has been adduced in support of its existence, and that all analogy is opposed to the notion that such an infinitesimal dose—not being a living organism with powers of reproduction—can have any effect whatever on the human frame.

There is one experiment in homœopathy, at any rate, which, notwithstanding their alleged disinclination to test it, doctors have frequently performed. Innumerable medical students, I suppose, have in joke, or from curiosity, taken homœopathic globules—I have seen a bottleful swallowed at a time—and their unvarying testimony has been that they have remained unconscious of the slightest effect from these agents, alleged to be so potent. Nay, the effects of these globules, taken singly and in numbers, have been tested by observations on the temperature, blood, pulse, respiration, &c., by all the instruments of precision which we possess, and with this result that not one trace of their influence can be detected. Now, in all the small-dose agents that have been adduced in order to suggest the possibility that decillionths may be efficacious—although, indeed, their small doses are as incomparable to decillionths as the National Debt is to half a farthing—in all these small-dose agents—animal and vegetable poisons—the effects of their administration to healthy human beings or animals are marked and determinate, and the nature of the poison may, in most instances, be inferred from an observation of their effects. They are subject to experiment, but the decillionth is not.

Homœopaths admit that their infinitesimal doses have no effect on healthy human beings, but they add that these are nevertheless mighty in counteracting disease. Is disease, then, some new thing introduced into the body, sensitive to influences to which the body itself is insusceptible? Assuredly not; no, not even in those instances in which it

is caused by micro-parasites in the blood ; for the micro-parasites are not the disease, but the cause of the disease, acting at close quarters, and, by the material ravages which they cause, or the poisons which they generate, disturbing the functions of organs and tissues, and the rational treatment of a micro-parasitically induced disease is not by similars but by contraries, by antidotes or antagonists, as in the case of toxic agents, by whatever will kill, extirpate, or destroy the parasite, check its growth, limit its productiveness, or counteract its effects. It may be proved to homœopaths that their decillionths have no effect when directly applied to colonies of pathogenic bacteria outside the body, and how infinitely less than nothing must be their effect when the decillionth is diluted with the bulk of the blood, and when the bacteria are scattered through the body in countless myriads? Can the homœopaths mention a single symptom of disease which is not a modification of a natural function, a single morbid growth not made up by histological elements that exist in health? They cannot, and they know they cannot. Upon what, then, do their infinitesimals act? On the imagination, and on that alone. They allow that on healthy persons they have no effect, and a diseased person is a healthy person out of tune, but with no new chords added which might be responsive to more delicate vibrations than the normal instrument is capable of responding to. Now, imagination has a recognised place in the sick room, as in science. It may be an excellent aid to treatment, but it can never take its place, except in fictitious ailments. What would a coroner's jury say to a practitioner who trusted to imagination in the case of a man who had taken a poisonous dose of arsenic and did not use the proper antidotes? And yet a conscientious homœopath, who restricts himself to homœopathic doses, must frequently stand by and see a man sink under organic changes quite as gross and conspicuous as those caused by arsenic while he trusts to imagination for a cure.

Notwithstanding all this, however, homœopaths come to doctors and ask them to try their system, and Lord Grimthorpe is wrath because doctors will not listen to their appeal. Try it, they say, and observe its effects in certain diseases ; and these certain diseases are always the most trifling or most obscure, for surgery they will not touch, nor certain maladies which yield readily to external application. And the answer to their appeal is, Why should we try it? Show cause why we should put aside the remedies in which we have learnt to trust, and experiment on our patients, who lean on us for life and health, with your infinitesimals, which are discredited by all that we know of physical science, of physiology, and pathology. Your empirical argument that they do good is just that which is employed in favour of every quack remedy. Perkin's metallic tractors were certified to have done good to thousands, but they are forgotten. St. John Long produced on his trial a long list of nobility and lawyers who were his patrons and grateful patients, and Lord Ingestre swore that he had seen him draw several pounds of a liquid like mercury from a man's brain : but he died of the disease which he professed to cure, and his nostrum is lost in oblivion. Not a day passes that quack remedies are not brought to us, with loud-sounding praises of their restorative power. Are we to try them all, even although there is not a presumption in their favour? If a substance has distinct physiological

actions, if it is allied to other substances the utility of which is established, or if it comes to us recommended by an observer of known probity and scientific attainments, it may be entitled to a trial; but as for your decillionths, it would be folly to meddle with them, more especially as many of the diseases in which you propose to use them have a tendency to spontaneous recovery which would make any limited number of observations, or any observations not carried out under the strictest conditions, exceedingly fallacious.

Then, further, say doctors to homœopaths, your remedies have been tried again and again by those in whose sagacity and fairness we have complete confidence, and they have pronounced against them. Why should we persevere with the fruitless inquiry? Moreover, they go on, you homœopaths do not, as a body, impress us so favourably as to dispose us to put much confidence in any representations you may make. You evidently do not trust in your own system, for when you have really dangerous illness to treat you adopt ours. (I have before me at this moment a list, furnished by a well-known druggist, of prescriptions recently dispensed for practising homœopaths for the following drugs, all in full ordinary doses:—Cocaine, extract of hemlock, hydrastis, iodide of sodium, oxide of mercury, and boracic acid.) And not only do you frequently desert your ship under stress of weather, say the doctors to the homœopaths, but you are widely at variance among yourselves, and not rarely display concomitant variations of homœopathy which fills us with suspicion. Some of you are mesmerists as Hahnemann was, some are vegetarians, and some, in America at any rate, are addicted to fanatical phases of religious belief.

Last of all, say the doctors, we have looked into your system, and have considered the methods on which you proceed, and these we are able to declare, from our own daily experience, to be absurd and unworkable. No drug can, as you assume, produce a picture of any disease, or even such a rough sketch of one as would for a moment deceive a practised eye; and as for your provings they are a maze of bewilderment. I write this letter in the country, as I have done two former ones, without access to books, but I am certain I am not exaggerating when I say that Dr. Dudgeon gives among his provings of aconite the following observations, though not in these exact words: —Face ghastly, face deadly pale, face pale, face pallid, face pink, face red, face crimson, face turgid, face livid, face dusky, and so through all other provings; and yet by these a homœopath is to be guided in his administration of the drug.

"A Scceptic," whose somewhat sanctimonious air of superiority is worthy of a true believer, alleges that I have busied myself with the "fringes" of homœopathy, and if I have done so it is because I have found no texture in it. It is "all ruffles and no shirt." I trust, however, that I have in my "forensic manner" somewhat frayed the already tattered fringes. And is not "A Sceptic" on a fringe himself in his elaborate endeavour to show that "R. B. C." and I differ about the sense of smell and have not the scientific habit of mind? But I beg to assure him that his ingenuity has been thrown away, and that we do not differ at all. In writing about that sense I added the word "gases" to Michael Foster's "odorous particles" for the

express purpose of making room for that "undulatory theory," if I may so call it, which "R. B. C." has stated with such rare brevity and lucidity, for it seems probable that the motion which excites smell, if the undulatory theory be correct, is propagated by air, which is a mixture of gases, from the odorous body to the peripheral expansion of the olfactory nerves in the nostrils. But I would point out to "A Sceptic" that the particulate and undulatory theories of smell are not exclusive of each other. The distances at which odorous bodies act in exciting the sensation of smell are very varied. Some are scented a mile off, and others not until they are in actual contact with the mucous membrane of the nose. But nerve endowment is invariable, and whatever the stimulus may be; and therefore particles or waves of motion impinging on the nerve ends in the nostrils, smell may equally be induced. A scratch of the optic nerve will cause a sensation of light as well as the undulations of luminous ether, and solids, liquids, and gases alike excite the nerves of common sensibility. But the nature of the external excitant of the sense of smell, whether that be movement in air or ether or particles in motion, is not, as "A Sceptic" sophistically alleges, "a point which lies at the very threshold of medical science," but a point which lies entirely in the domain of the physicists, and to them I refer him, merely remarking that perhaps in the phenomena of the absorption of light or of phosphorescence or fluorescence he may find some analogy to the scent left by animals on their trail which has perplexed him and Dr. Millican so sorely. It is to be regretted that "A Sceptic" has not dipped into some physiological book, which would have saved him from asserting that we know nothing of smell, and from writing about it as if the sensation was something outside the percipient subject. Of course, the smell of musk may bring on a headache; we have been told that a man may "die of a rose in aromatic pain," but we also know that the most horrible stenches may be felt where they have no existence because there is disease of the brain or irritation of the olfactory nerves.

And we are favoured with this long disquisition on smell because Dr. Millican chose to compare incomparables—a nerve stimulus and the therapeutic powers of a decillionth of a grain. Taking him on his own ground and his particulate theory I endeavoured to reduce his speculation to absurdity, and did not, therefore, enter on the undulating theory. And the whole discussion is really beside the question, for the best of our homœopaths have now given up decillionths and admit all Hahnemann's attenuations to have been preposterous. They have abandoned the itch miasm as the cause of seven-eighths of disease. They have abandoned the spiritual potency conferred by succussion and trituration. They have abandoned the infinitesimal dose. What, then, remains of this disintegrated creed to its more enlightened followers? Nothing but the *similia similibus curantur* theory, and that many of them admit to be only of partial application. The instances they rely on, for even this partial application, can all be explained on other hypotheses, and it is to be hoped, therefore, that the enlightened homœopaths will yet quit a camp in which they have some shady companions.

One excellent result this controversy has had, in driving homœo-

pathy to take up its right place among the other forms of advertising empiricism. It has for some time been deluging the country with trumpery tracts, distorting the opinions of leading members of the medical profession, and it has now taken to advertising its cures, as your columns on Saturday bore witness. We have them wholesale at first, we shall have them in detail by and by. By the adoption of the tactics of our enterprising soap-boilers, by flaming posters and funny puzzles widely distributed, homœopathy may retrieve its fallen fortunes and do business for a time. But this, again, must lead to cleavage, for it is impossible to believe that the men of "high intelligence and culture" of whom Dr. Bucknill speaks, learned homœopaths, homœopaths by early inculcation, or those who have in youth rashly taken up a theory which they never wholly believed, will consent to be dragged through the mire by their less nice and more commercial associates.

I shall not follow Lord Grimthorpe in his desperate plunges about the ring, nor ask the umpire's opinion on his style of hitting—as, for instance, in carrying through what I am glad to see he admits is "a dirty job," in representing that his calculations of the number of homœopaths in the world at 10,000, and in America at 11,000, referred to different dates. Both these calculations clearly referred to the present time, and both are outrageous for any date that he may choose to fix. He was warned not to trust these mendacious pamphlets, and he now sees what comes of neglecting advice. I wonder how he looked when he read Dr. Bucknill's and Dr. Brunton's letters this morning, upsetting, as these do, two of his most effective and elaborate paragraphs, and I wonder still more that he should have the coolness to assume that I am going to bestow a thought on his collection of "marvellous homœopathic cures in cases given up by the faculty." In any provincial paper he will find columns of such cases, far better than his, bolstering up the virtues of soothing syrups, and oils, and balsams without end. It is good sport to contend with Lord Grimthorpe; but I am tired of it, and so, I daresay, are your readers. Let me disclaim, however, having spoken of Mr. Proctor, for whom I have the highest esteem, as "a slipshod scientist" except in irony. I have remarked, however, that weapons finer than the bludgeon are scarcely noticed by Lord Grimthorpe. What I meant to convey was, that in Lord Grimthorpe's estimation all scientists, even the best of them, are slipshod and stumble, and that only lawyers and lords are fully booted and spurred, and walk unerringly.

I have declined already to discuss with "A Sceptic," except in a medical journal, the actions of drugs, and I still more strenuously object to discuss with him their non-actions, to the consideration of which he now invites me. Neither with him nor with the homœopaths, in search of a dose, shall I consent to grope my way through the endless reaches of nonentity. I have even resisted the temptation, which some unwary expressions in his letter held out, to excellent fun at his expense. I can assure him, however, that his difficulties are puerile, just such as assail the medical student in his second or third year, to be brushed away by riper knowledge. Singularly enough, he has selected as an illustration of medical ineptitude, one drug, the nitrite of amyl, which was brought into use by a physio-

logical study of its actions, which has incontestably saved and prolonged many lives, and given relief to innumerable sufferers, who but for it must have endured the bitterest anguish. In the presence of such facts his cavils are shallow and ungracious.

"One of the Laity," who ought, I think, to date from Trafalgar-square, exhorts all true-hearted Englishmen to subscribe to the Millican Defence Fund, without, however, mentioning the amount of his own contribution, and declares that Dr. Millican is making a noble stand for liberty of opinion. Who, I should like to know, has attempted to curtail the liberty of opinion of Dr. Millican, whom, notwithstanding my "venom," I regard as a very able man, although not exactly a west-end Hampden, or of any other medical pervert of any sort? They are free to think, to speak, to write, to practise as they please, no man gainsaying them. Is it an interference with liberty of opinion that doctors will not consult or hold professional intercourse with homœopaths, whom they believe to have adopted opinions so erroneous as to vitiate their professional action and judgment at every turn or to be conscious charlatans? Does "One of the Laity," like Lord Grimthorpe, propose to remedy this by a new dispensation which might be called the Boycott-positive to distinguish it from the Boycott-negative, the Irish form? Does he propose that doctors shall be compelled to have dealings with homœopaths, on all occasions, and under police supervision if need be, that they shall meet them at the bedside of their private patients and in their hospital wards, swallow their pilules, elect them honorary members of their societies, and invite them to dinner? Then, why stop at homœopaths? Why should not the liberty of the true-hearted Englishman be extended to herbalists and bone-setters and the pill men in the booths at the fairs? There is much craziness outside Bethlem and Colney Hatch.

Unless Lord Grimthorpe springs up in some wholly unexpected quarter, I shall not trouble you again on this odious subject.

J. C. B.

DR. G. JOHNSON (2).

[*Jan.* 18.

Lord Grimthorpe does not see what I have to complain of, unless the homœopathic tract, "Allopathy Judged by its Professors," invented the following utterance:—"The most general and comprehensive statement with regard to the cure of disease which can safely and confidently be made is this—most of those diseases which are curable by any means are curable by the unaided powers of nature." The author of the tract, which I have not seen, but which Lord Grimthorpe gives as his authority, does not complete the sentence which he professes to quote from me. The sentence concludes as follows:—"And the chief art of the physician, as of the surgeon, consists in regulating and directing those natural forces which will cure a fever or an inflamed lung as surely and as completely as they will heal a wound or mend a broken bone." From this it will be seen that my patients are not consigned to the "unaided" powers of nature. What, therefore, I have to complain of is, that in Lord Grimthorpe's former letter I am made to say that *every* curable disease is cured by nature if we do not

manage to prevent it; whereas what I actually said was that *most* curable diseases are curable by the unaided powers of nature, while even with regard to them the duty of the physician is to regulate and direct the natural forces.

The lecture in question was addressed to a mixed audience of teachers and students of medicine, who, in the course of the same lecture, were told, what most of them knew before, that many diseases can be cured only by the discovery and removal of their exciting causes, while others are to be removed by the employment of special remedies and methods of treatment. I should feel thankful to Lord Grimthorpe if he would reprint the whole of that lecture, which, however, would not be pleasant reading for his homœopathic allies.

It is much to be regretted that a man in the position of Lord Grimthorpe should quote as authoritative and trustworthy an anonymous pamphlet containing extracts (in my own case a garbled extract) from the writings of well-known physicians—extracts so detached from the context as to convey an entirely erroneous impression of their author's doctrines.

Believing as I do that the columns of a newspaper are unsuited for the discussion of the respective merits of homœopathy and scientific medicine, I should have taken no part in this discussion, but that, having been personally referred to, I felt called upon to protest against the flagrant unfairness of homœopathic controversialists. But now, having made this protest, I beg permission to refer briefly to a part of the subject in which the general public have an especial interest, and which they can easily be made to understand. One of the leading doctrines of Hahnemann was that remedies are to be given in infinitesimal doses. Now, it is notorious that this doctrine, so opposed to common sense and to the general experience of mankind, has been abandoned by many modern homœopaths, who have passed from the irrational and ludicrous extreme of infinitesimal dilution to the opposite and dangerous extreme of employing active and poisonous drugs in the form of the greatest possible concentration. Of the alarming effects of one of these homœopathic poisons I have had considerable experience. I refer to the so-called "homœopathic or Rubini's concentrated solution of camphor," which is in very general use for the treatment of colds and other ailments. This preparation is a saturated solution of camphor in spirit, and is more than seven times stronger than the spirit of camphor of the British Pharmacopœia. In a recently published volume of "Medical Lectures and Essays" I have given the histories of nine cases of poisoning by this dangerous preparation. The slighter symptoms, resulting from doses of from three to seven drops, have been giddiness, headache, faintness, and drowsiness; but in five cases out of the nine a dose of from 15 drops to a teaspoonful caused violent epileptiform convulsions and profound stupor. It is probable that in more than one case death would have resulted if the poison had not been speedily ejected by vomiting.

The patients and their friends were greatly surprised to find that such alarming symptoms should have been caused by a homœopathic medicine, since they had been led to believe that all such preparations were largely diluted and therefore harmless. The preparation in question is as potent a poison, drop for drop, as the prussic acid of

the Pharmacopœia, and it ought never to be sold without a "Poison" label on the bottle.

It surely is not too much to ask for some explanation of this substitution of concentrated poisons for the infinitesimal dilution which has been generally supposed to be one of the essential dogmas of homœopathy.

GEORGE JOHNSON.

DR. THOS. WILSON.

[*Jan.* 20.

Magna est veritas et prævalebit. Truth is what the disciples of Hahnemann wish for; if homœopathy is wrong, it will soon die a natural death; if right, no persecution can put it down.

There is nothing new in the doctrine of similars. See the "Genuine Works of Hippocrates," translated from the Greek by Francis Adams, LL.D., surgeon, 1849. On page 77, vol. i., the author remarks:—

"The treatment of suicidal mania appears singular—give the patient a draught made from the root of mandrake, in a smaller dose than will induce mania. He then insists in strong terms that, under certain circumstances, purgatives will bind the bowels, and astringents loosen them, and he further makes the important remark that, although the general rule of treatment be *contraria contrariis curantur*, the opposite rule also holds good in some cases—namely, *similia similibus curantur*. The principles both of allopathy and homœopathy, it thus appears, are recognised by the author of this treatise. In confirmation of the latter principle he remarks 'that the same substance which occasions strangury will, sometimes, also cure it, and so also with cough.' And, further, he acutely remarks that warm water which, when drunk, generally excites vomiting, will also sometimes put a stop to it by removing its cause. He estimates successful and unsuccessful practice according to the rule whether the treatment was rightly planned or not. For, he argues, what is done in ignorance cannot be said to be correctly done, even if the results are favourable."

Surely when the father of medicine declares both practices, allopathy and homœopathy, to be correct, the two schools need not quarrel over the matter, as each is right after its own way; the fact is that both rules hold good when correctly applied. But, how difficult it is to get a contrary or opposite to a disease under drug treatment! Certainly you may give an astringent in diarrhœa, or an opiate to procure sleep. How will you find a contrary to a skin disease? The homœopathic provings of drugs indicate their power in producing eruptions on the skin. Thus, tartarized antimony produces eruptions of a pustular nature, outwardly applied or taken internally. In the treatment of smallpox, tartarized antimony in minute doses is invaluable.

Some years ago I was much puzzled in the treatment of a severe case of *sycosis menti*—a pustular eruption on the skin. I treated the case for months without benefit, not having selected the true homœopathic remedy. My patient discontinued attendance, and I considered the case incurable. One day, in looking over the homœopathic symptom-

books, I came across the extraordinary powers of tartarised antimony in producing pustular eruptions. Some time afterwards I met my patient in the street, still frightfully disfigured with the disease, I advised him to call on me again, as I thought I had discovered the means to cure him.

Once more he became a patient. I commenced the tartarised antimony with the best result. In a few weeks he was perfectly cured, and never had a relapse.

Why should allopaths find fault with the small dose and say it is impossible for infinitesimals to cure disease? When it is simply a matter of experience, the small dose frequently cures much quicker than large ones—nay, more, removes the complaint when a large dose of the drug has failed.

If any allopath wishes to test the power of a small dose on himself, let him get an ounce of the third centesimal trituration of calomel, made correctly after the plan of Hahnemann. Let the same allopath take steadily one grain of the preparation dry on his tongue night and morning until the whole is consumed. Each grain only contains the millionth part of a grain of calomel. Take my word for it that he will cry *peccavi* long before he has finished his ounce of calomel tituration. If he escapes salivation it will be fortunate for him, and will only show that he is not easily susceptible to the action of mercury. To make the experiment sure, let one dozen allopaths begin at the same time to take a grain of the preparation night and morning, and report the result to one another. If they try the experiment fairly they will have much to say afterwards on the subject, when each has completed his ounce of powder.

We are all still extremely ignorant of the powers of nature. Every year brings forth something new and wonderful.

Who can explain why a powerful horeshoe magnet can magnetize a bar of steel and lose no power, in fact, improve by the operation? One may continue to magnetize steel with the same horseshoe magnet for ever and ever, until all the steel in the world becomes magnetic, and still the magnet remains as lively as ever.

How presumptuous of anyone to assert that infinitesimal doses of drugs are inert, until he is acquainted with all the secrets of nature!

What quantity of scarlatina poison will a person inhale who receives a letter from a house where scarlatina exists? And yet a patient of mine took the complaint after the receipt of such a letter, when no other cause could account for the disease.

In the strictest sense of the word, I am not a homœopath nor an allopath. Having used homœopathic remedies for over forty years, and having had medical experience for more than fifty years, I still prescribe from my fifty years' experience, and take the entire range of medical knowledge as far as my experience has gone—that is, sticking to the bridge that has safely carried me over before.

My opinion is that far too much reliance is placed on drug treatment alone. We all know that thorough cleanliness, free ventilation, and the avoidance of all noxious effluvia contribute greatly to health. Our hospitals are kept in a much better sanitary condition than they were forty years ago, but are still in many cases overcrowded. The old-fashioned plan of darkening the rooms of the sick—dungeon-like—did

much to prevent recovery. The light of the sun is very beneficial in sick apartments. There are other great aids to removing disease, such as manual magnetism, massage, electricity, hydropathy, change of air, climate, &c.

I must apologise, Sir, for the length of this communication; but I find homœopathy completely boycotted in allopathic journals, which must plead my excuse.

Above twenty years since, the senior physician to the Hull General Infirmary became a convert to homœopathy, and asked the governors of the hospital to let him have a ward to himself. This was refused, and he was so boycotted by the remainder of the medical staff that he was compelled to resign. Perhaps they considered "comparisons odious."

Withernsea, Hull. THOMAS WILSON, M.D.

FORTY-TWO YEARS OF HOMŒOPATHIC TREATMENT.

[*Jan.* 20.

The battle is still raging between the doctors, under the able supervision of Lord Grimthorpe. The doctors must have nearly exhausted themselves, but so little has appeared from any of the dupes of the homœopaths that I ask you to let me give my experience of forty-two years under the impostors.

My friend "R. B. C." has reduced the question to a sharp issue. "To put the matter quite plainly," he says, "its absurdities, to any who have received a medical education, are so manifest, that we do not believe such persons, if of ordinary mental capacity, can honestly believe them, and we have abundant evidence that many who profess to be homœopaths do not carry their professions into practice, but prescribe ordinary remedies, and only call themselves homœopaths, in order to fleece some of the more gullible sections of the public. To the genuine homœopaths, if such there be, we object because we distrust their mental capacity, and to those who are not genuine we object on grounds of ordinary morality."

The case could not be more clearly stated. Forty-two years ago my brother, having attended my family for some years and having given 12 months' study to homœopathy, announced to us that he was convinced of its truth, but offered to treat us by either method. We told him that we trusted in him, and preferred to be treated by what he considered the best method. He is a man of whom Emerson said some years afterwards, in his "Representative Men," that he had one of the greatest minds since Bacon. He had also received a medical education, and he thereby falls necessarily into "R. B. C.'s" classification as a consummate scoundrel. My dear brother and I are old men now, and it is too late for him to recant, after his 42 years of successful scoundrelism. For 25 years his attendance on us and our children endeared him to us for his successful treatment, and it is only now that we find what a villain he has been.

For fifteen years, when we removed to another neighbourhood, we have had the admirable talents of one of our dear friends in a similar capacity. Indeed, he is attending me now for an attack of gout (pulsatilla, 3d dilution). He, too, I find, is another of "R. B. C.'s" rascals. During the whole of these forty-two years my wife and I

have been constantly making use of homœopathic remedies upon our children and grandchildren, and have, with loving eyes, observed their wonderful efficacy. Can parents be deceived for forty-two years, or is it possible that "R. B. C." can be wrong? Are we rogues, too, or only fools? Have we and our children benefited by homœopathic treatment, or have we only foolishly thought that they did?

Another of these scientific allopathists, who is the Jubilee Hospital doctor, is also in the good company of "R. B. C." when he says that "every homœopathic practitioner is a conscious fraud, a liar, and an impostor." Such people ought to be very sure of their ground, and to be sure that they speak from a true scientific standpoint, of obvious truth, and generally accepted among their fellows. Dr. Wilks says, "I deny that we have a scientific use of medicines." Sir Astley Cooper said of them in his day, "The practice of medicine is founded on conjecture, and improved by murder." It was of an allopath that it was said, "Sir, if thou hadst *not* been here, my brother had not died."

Homœopathy, on the contrary, is a system and a science, and, like all true science, is constantly enlarging its boundaries, and it may enlarge its doses, too, when it finds occasion, its grand principle not being affected by the quantity of the remedy. One would think, to hear these gentlemen talk, that it is the homœopathists who invented death. It is not only they who have to "carry their work home," but, happily, they do it much more seldom than the others.

One weakness, however, it appears they have, and that is their wish to be associated with the old school. In my opinion, they ought to be ashamed of being seen with them, until the old school mends its practice, as well as its manners. The sooner they take to saving life by giving nothing but sugar globules the better it will be for their patients.

44, *Lincoln's-inn-fields.* W. M. WILKINSON.

MAJOR W. VAUGHAN MORGAN. [*Jan.* 20.

Will you permit me to state for the information all who sympathise with freedom of action in medical matters that I am the treasurer of the fund mentioned by Dr. Thudichum which is being raised to assist Mr. Millican in his gallant stand against medical bigotry?

I would also venture to ask your assistance in making known that, in addition to similar institutions in Liverpool, Birmingham, Bath, and elsewhere, there exists in Great Ormond-street a hospital containing upwards of ninety beds, where every species of non-contagious disease is treated. There all inquirers are welcomed, and scores of patients of all ages and both sexes may be seen under treatment.

Is it too much to hope that "R. B. C.," "J. C. B.," and others who have recently been displaying their ignorance of homœopathy may seize the opportunity of seeing and testing its actual practice? The members of the staff of the hospital will gladly explain everything to their medical *confrères*, and students can receive clinical instruction in the wards. In the dispensary the mode of preparing the medicines can also be witnessed, and the absurdities of "R. B. C.'s" statements be proved by ocular demonstration. I abstain from touching upon professional matters.

WM. VAUGHAN MORGAN.

5, *Boltons, S.W.*

"S."

[*Jan.* 20.

Fighting yet floundering, hitting wildly at his men of straw, your correspondent "J. C. B." displays a marvellous ignorance of the subject of homœopathy. The homœopathy he threatens to expose, and for believing in which he denounces some of his fellow-practitioners as impostors, is evolved, in a great measure, out of his own inner consciousness, and is thus the more easily demolished to his entire satisfaction. How often is it to be denied that the infinitesimal dose is not an essential part of the creed of a homœopath; how small the dose may be is a matter of experience.

But I take exception to his statement that "that remedy can only be called homœopathic our knowledge of which we owe to homœopathy, or which is employed in homœopathic attenuation;" and claim that that remark alone shows to anyone who has the least knowledge of the subject that "J. C. B." has not in any way touched the bottom of his opponents' arguments. No drug is in itself homœopathic, such an expression is a manifest misnomer; its homœopathicity consists in the method in which it is applied for the relief of disease. Let me take one example. Calomel was a favourite remedy long before Hahnemann's time, and its action known and noted; but homœopathy can show an additional and valuable use of that drug (viz., in certain forms of diarrhœa) without excluding its allopathic action, or without its being used in the so-called homœopathic attenuation.

The scouted "sterile toils" of the homœopaths have, notwithstanding "J. C. B.'s" severe criticism of their labours, worked a marked change in the administration of drugs during the past twenty or thirty years, and have placed in the works of the orthodox therapeutical writers of the day, loth as they are to acknowledge it, some of their most valuable hints for the relief of suffering and disease.

S.

At this stage of the controversy *The Times* found it necessary to close its columns to further letters on the subject. The entire substance of the matter is reviewed in a leading article which appeared on January 20, and is here reprinted.

LEADER FROM *THE TIMES* OF JANUARY 20.

Lord Grimthorpe must be highly delighted with the results of the discussion he started in our columns. In the first place, it has been a very lively one, and he loves animation. In the second, it has excited an immense amount of public interest, which, we presume, is a gratifying circumstance. We have given what will be admitted to be a liberal allowance of space to the correspondence, but the letters we have been able to insert represent a mere fraction of the number we have received from all sorts and conditions of men. In the third place, Lord Grimthorpe has the satisfaction of reflecting that he has been entirely successful in establishing his original contention. So wide is the field over which the discussion has travelled, that it is perhaps necessary to remind the public what the original contention was. It was simply that an *odium medicum* exists, exactly analogous to the *odium theologicum* of a less enlightened age, and no whit less

capable of blinding men otherwise honest and kind-hearted to the most elementary conceptions of candour and justice. That contention has been proved not so much by what Lord Grimthorpe has directly advanced as by the revelations of temper and mental attitude made by those who took up the cudgels on behalf of the orthodox profession. There have been one or two verbal denials of the existence of this *odium*, always accompanied, however, by an expression of contempt which comes in practice to much the same thing. But the strength of Lord Grimthorpe's case lies in the fact that whole columns have been filled with contentions which have no point or meaning except to justify the hatred that is verbally denied. Homœopaths are fools if they believe and practice what they profess, and knaves if they do not; therefore, we are justified, and, indeed, bound, by the lofty considerations which alone influence professional action, to hate and despise them in either case—is a fair and accurate summary of the attitude assumed by orthodox champions at the opening of the discussion, and maintained with unswerving consistency up to the present moment. But that is the precise attitude which Lord Grimthorpe intended to describe by the phrase *odium medicum*, and, therefore, out of all the confused discursiveness of the controversy emerges the fact that he has amply justified his main and original statement.

We do not know exactly what end our orthodox correspondents have proposed to themselves, consequently it might be unscientific upon our part to express any positive opinions upon their mode of conducting the controversy. If they wrote merely to relieve their feelings and comfort those who already agree with them, they probably have every reason to look complacently upon their own performances. But if they either desired to convince homœopaths of the greatness of their delusion or sought to enlist the sympathy and command the confidence of the lay public, we are quite sure that they have made an egregious mistake. At an early stage of the controversy we tried to hint as much to our professional advisers and guides. We pointed out that it is a mistake to fling charges of knavery and folly, either alternatively or cumulatively, at men taught by the same teachers, trained at the same schools, and declared qualified practitioners of medicine by the same authorities as themselves. To call a man a fool who holds exactly the same diploma as the men who abuse him merely because he differs upon some medical subtlety which laymen are told they cannot form an opinion about, has the effect of filling the lay mind with distrust of the very certificates upon the strength of which the doctors challenge our confidence. If one M.D. duly licensed by an orthodox faculty can be such a fool and as nearly a criminal lunatic as his brethren make him out, poor laymen cannot but feel that there may be other wolves in sheep's clothing passed by the same authorities, and all the more to be dreaded because they carry no distinctive badge. When doctors are denounced as knaves whom laymen have known all their lives, and who, in all the ordinary relations of life, behave with quite average common-sense and integrity, it becomes rather difficult to repose implicit confidence in some practitioner whom we only know by name, merely because he professes utter disbelief in the efficacy of decillionths. When our orthodox friends descend in their wrath to the practices of the tenth-rate politician, and pick up

any bit of malicious gossip at second or third hand—the chatter of a discarded servant or the loose statements of an anonymous but necessarily interested druggist—it is hard for the ordinary layman, who does not readily rise to their temperature, to feel very deeply convinced of the sobriety and trustworthiness of their judgment. We poor laymen are painfully aware of our natural deficiencies, and, if we were not, we have been reminded of them most forcibly and frequently. Some laymen have taken part in this controversy, and have shown what seemed to other laymen a certain degree of knowledge. But they have been summarily dismissed as persons destitute of qualification for discussing those high matters, and all of us have been admonished that our only safety lies in choosing a good doctor and placing ourselves unreservedly in his hands. It is clear that we cannot choose him on medical grounds, because we are unfit to understand them. Our intelligence has, indeed, been flattered at great length by the assumption that we are competent to pronounce infinitesimal doses absurd, but then other things have been mentioned which look quite as absurd to the lay mind, and which we have to accept as the infallible conclusions of science. No guide remains for us except common-sense operating upon considerations such as we are familiar with in our ordinary affairs. Consequently, a real injury has been inflicted upon us by those orthodox practitioners who have so conducted this controversy as to arouse in every unprejudiced lay mind the horrible doubts to which we have just referred.

When we last wrote upon this subject it was already evident that the controversy covered a much wider field than that of Lord Grimthorpe's first letter. It had become a dispute between two systems or schools of medicine. Being only laymen, we are of course incompetent to hold a rational opinion upon such a subject, but it was open to us to endeavour to get the controversy conducted in accordance with the general rule that disputants ought to deal with the arguments of their opponents as stated by themselves, not with any loose travesty of these arguments that prejudice or ignorance may suggest. We accordingly took some pains to ascertain and set forth the homœopathic position as stated by homœopaths themselves, and we were afterwards encouraged to believe that we had done so with—for laymen—tolerable exactitude. It ought not to have been necessary, because every orthodox practitioner ought to know the best as well as the worst of homœopathy, and every orthodox controversialist ought to be ready to state his opponents' position accurately and fairly. It was necessary, however, and we did it, but without the slightest effect. Orthodox writers went on through column after column blazing away at what is non-essential, accidental, and extrinsic, while the essential points upon which the whole argument turns were left untouched. What disquisitions we have had about decillionths, and how utterly irrelevent they are when homœopaths maintain that dose is a mere affair of experience and that the essence of their system is a rule of drug selection based upon observation of the effects of drugs upon the healthybody! Their rule may be rotten and worthless, but we can never advance one step towards proof of that fact by losing ourselves in calculations concerning the space that a decillion of grains woud occupy. A correspondent tells us to-day that the cases in which like

seems to cure like can be explained upon some other hypothesis, which he does not mention. But that is not the point. Homœopaths do not offer any explanation or hypothesis. What they say is that the rule leads them to the choice of the right drug for a given case, and if that is so it does not greatly matter although what they call likes are really wide as the poles asunder. The same correspondent tells us that infinitesimal doses have no effect upon a man in health and therefore can have none in disease. Here he rather trenches upon the domain where even a layman can check him. When a layman has an inflamed eye, he finds that it will not bear the ordinary daylight in which he rejoices when his eye is well. When his nervous system is out of gear, he is driven nearly mad by noises which do not affect him in health. When he is recovering from an illness, his stomach will not bear the solid food he finds necessary at other times. It follows that whatever is based upon our correspondent's *dictum* manifestly stands upon a quicksand. Another correspondent says to-day that if anybody likes to try the effect of one-millionth of a grain of calomel three times a day, he will find that it is unpleasantly potent even in health. The effects produced are the ordinary physiological effects of a dose of calomel, and the experiment may be tried by anybody in his own person. How much less than a millionth will do we cannot say, nor do we know whether the millionth would be more active in disease. These are matters of fact, and we mention them only to show that we laymen have not had that assistance from our orthodox friends which we might fairly have expected.

APPENDIX.

Containing certain letters sent to *The Times*, but not inserted; and also the letters headed HOMŒOPATHIC CHEMISTS, really belonging to the controversy, but not published until after it had been brought to a close.

LORD GRIMTHORPE (8).

Simultaneously with the above leading article, and therefore covered by it, there appeared two more letters from "J. C. B." and Dr. C. Johnson. The first I have not read beyond glancing at the style of it, and casually seeing that he tried to shuffle out of the exposure of his ignorance in calling a great astronomer "a slipshod scientist" by pretending that he meant it "ironically," expecting us to forget that that would have made absolute nonsense of it. In short, that was another specimen of "medical ethics." One need read no more from such a man.

After *The Times* had closed its correspondence, and though Dr. Johnson had had the last word in it, he began a fresh correspondence with me, sending me a copy of his lecture, which, of course, was correctly quoted in his letters in *The Times*. I was rather amused to see that it ended with the much better quotation, "Prove all things: hold fast that which is good." Remembering the efforts that his party have always made, and are making every day, to prevent any proof being given to the public of what is good, even to suppressing the truth whenever it has been given, as in that disgraceful cholera business in 1854 after which I do not see what claim they have to be believed in any general assertion on any subject. Dr. Johnson in one letter intimated that he might publish our continued correspondence, to which I answered, "Do, by all means, if you like," but in his last he changed his mind. It is too long to add here, and I can say all I need by way of remarks on his last letter to *The Times*.

It is strange that any man could think such a great question as this can turn in the smallest degree on whether he had told his lecturees that *all* "diseases that are curable at all are curable by the unaided powers of nature," or *most* diseases. For either of those statements are a plain confession that the only medical treatment he knows of generally does no good. And the more so, because I only quoted him, together with above a dozen more eminent authorities, all saying substantially the same thing in various phrases and with varying intensity, from his up to those who said it would be better for mankind if there were no doctors at all. Of course no one quotation could be verbally accurate for all of them. I would gladly have given them individually, but for the space it would have required.

His last letter in *The Times* complained further that the 9th H. tract

had done him injustice by omitting another sentence, or part of the same, in his lecture, viz., that "the chief art of the physician consists in regulating and directing the natural forces, which will cure a fever, &c." His fresh correspondence gave me the opportunity of asking him whether he meant us to understand that there is any difference between "aiding nature" and "regulating and directing the natural forces." He confessed that he was not aware of any, and there obviously is none. Therefore, the whole oracle that he delivered was really this: "Most diseases that are not known to be incurable are curable by unaided nature, but the chief business of the physican is to aid nature"—or to do what is generally useless. If he had said that of the homœopathists, of course we should not be surprised; but saying it of his own craft was a degree of modesty which is only the more extraordinary to outsiders because it seems quite ordinary among these priests of Apollo at home among their own disciples. In fact, as it is indisputable that they do sometimes cure patients who would die without them, I conclude that what they mean by so depreciating their own art is that, if all drugs and doctors were cast into the sea, as one or two of them said they had better be, not quite the same individuals would die, but on the whole fewer.

I really don't know why he complained also, both in *The Times* and to me, that I had "intimated that his patients are consigned to the unaided powers of nature." I said not a word about them, and privately I was obliged to tell him that I certainly never suspected doctors of doing that, though, according to his own dictum, it might be better if they did. "And yet he is not happy." There is no pleasing some people. But what is more to the purpose is that he said in *The Times* that he had seen and mentioned in his lecture nine cases of homœopathic poisoning—to put it shortly (for he has the usual allopathic tendency to large doses of words), by "the so-called H. or Rubini's concentrated solution of camphor, which is in very general use for the treatment of colds, &c." He says probably several of them would have died but for spontaneous vomiting. Having the opportunity to ask about that also, I did ask whether any H. doctor or book had given or prescribed those doses; and again he admitted that he could not say, except that one of them, of about 20 drops, was given to a guide on the Alps by an amateur H., who had a bottle in his pocket from his own H. attendant. I then asked a H. doctor whether any of them would give such a dose as that to anybody, and he answered that none of them would dream of doing so, or half of it. And so end Dr. Johnson's nine convictions of homœopathic poisoning: or rather, so they don't end; for it is evident, as I told him, that the people who are really responsible for all such poisonings are the A. doctors, who go about assuring mankind that the H. medicines are only sugar and milk and water impregnated with nothing, or nothing beyond the proportion of a drop of poison in the Thames or the sea, and that consequently we may take a box or gallon of them without being either better or worse for it. They and their chemists, who dare not offend them, had been going on saying and "proving" this all through the previous correspondence while it was wanted. The moment the contrary is wanted, there is Dr. Johnson ready to prove that too; and he tops it up by saying that it is "*notorious* that Hahne-

mann's doctrine of infinitesimals had been abandoned by many modern homœopaths." So I asked him, and he did not answer it: Then what excuse remains for excommunicating them? I now begin to see that in allopathic ethics, "Prove all things," means "Prove all things that are wanted," for the moment.

G.

HOMŒOPATHIC CHEMISTS.

[*Jan.* 25.

Some serious reflections having been cast upon homœopathic pharmacists during the discussion of this subject, especially by Mr. Swann in a letter published in your issue of the 19th inst., we crave permission to make a few remarks in reply.

Mr. Swann would have your readers believe that not a single honest homœopathic pharmacist exists who has hitherto prepared globules, pilules, or tinctures "for trade purposes in this country according to the 'Homœopathic Pharmacopœia,'" and then proceeds to dilate upon the difficulties of preparing pilules and globules for "purposes of stock or sale," in a manner which shows an absence of the technical knowledge to which he lays claim, and which anyone acquainted with the processes of the "British Homœopathic Pharmacopœia" would characterise as nonsensical.

"R. B. C." and other orthodox practitioners have also from time to time suggested the impossibility of decillionths having been honestly prepared, on account of the enormous quantity of spirit which would be required for dilution. Now, any one who cares to make a simple calculation can verify the fact that one minim can be reduced to the proportion of a decillionth by means of rather over a quarter pint of liquid (12½ tablespoonfuls as stated by Mr. Ross), by preparing 30 small phials of attenuations each containing 1 per cent. of that previously obtained, as directed by Hahnemann—one minim of the strong tincture being thoroughly mixed with 99 minims of spirit, then one minim of the resulting solution being again treated in the same manner, representing 1 in 10,000, and so on through the series until the 30th is reached, representing 1 in a decillion (assuming matter capable of such degree of subdivision). Thus we use $30 \times 100 = 3{,}000$ minims = $6\frac{1}{4}$ fluid ounces = less than one-third pint of spirit.

If, then, for stock purposes, the pharmacist keeps the 100 minims of 28th attenuation, he can convert it when required into about 12½ gallons of the decillionth potency or 30th attenuation. Where, then, is the overpowering temptation to dishonesty which our detractors consider irresistible?

The analyses referred to by Dr. Dupré as having been made by him some fourteen years since show that the pilules in question were not made according to our present "Pharmacopœia," though they may have been conscientiously prepared by some older methods, perhaps unskilfully, and, in the case of the vegetable preparations, from drugs very deficient in alkaloid. Dr. Dupré is, however, in error when he states that "if as little as 1 in 800,000th part of a grain of aconite per pilule had been present it would have been detected;" he surely intended to write aconitine. He also says the pilules were made from

pure milk sugar, whereas pilules are in this country invariably made from cane sugar, but, as far as we know, such an error would not have affected his results.

We maintain that there are men among the homœopathic pharmacists of this country as honest and conscientious as any who are to be found in other professions, and we are personally acquainted with a considerable number of our *confrères* who are of this class, but as we believe the charges brought against us and our competitors too sweeping and unjustifiable to have any influence on the credulity of your readers, we will refrain from trespassing further on your space.

E. GOULD AND SON.

No. 59, *Moorgate-street, E.C.*

Will you allow me to inform Mr. Edward Gibbon Swann that it is quite possible to saturate pure sugar pilules with a tincture made with a diluted spirit, and that it is not necessary to steep the pilules in the tincture? (See "Homœopathic Pharmacopœia," page 40.) A little should be added at a time until saturation is effected, and if carefully done the pilules will not even become sticky. He also questions the directions of the "Homœopathic Pharmacopœia" as to the saturation of pilules with strong alcoholic tinctures. If he will take the trouble to purchase a small quantity of unmedicated pilules and treat them as directed on page 41, afterwards adding carefully in portions a sufficient quantity of strong alcohol coloured with sandal wood (this will give a dark-red colour, and will be found a good test as an experiment), he will find on cutting each pilule in half that the fluid has penetrated them thoroughly. Surely we have had enough of these statements concerning the honesty of homœopathic chemists. They are all false, and I shall be most happy to explain and demonstrate the foregoing processes to any person interested in the subject, whether physician or layman, who will call at our pharmacy.

L. T. ASHWELL,

74, *New Bond-street.* (Keene and Ashwell).

The following letter was written by Mr. Heath in reply to that of Dr. Dupré, but was not published in *The Times*:—

CHEMICAL ANALYSIS AND HIGH ATTENUATIONS.

I have no doubt the homœopathic chemists generally are much obliged to Dr. Dupré for so kindly exonerating them from the charge of selling unmedicated pilules and globules. It will comfort some people to know that an allopath of standing and ability admits that these preparations are honestly and properly made, but having said that they are medicated with dilutions containing 1-100 and 1-10,000, &c., why does he then imply that these very dilutions are not made correctly (it is as easy to make them correctly as incorrectly), it seems to me that he has placed himself on the horns of a dilemma. Now with regard to the dilutions, I think I can show how that the fact of

his not being able to detect aconite, by no means proves that there was none present in the 100 pilules he examined. The tincture of the first dilution no longer gives the tingling sensation to the tongue which is characteristic of aconite. Now the presence of aconitine could therefore only be proved in such a dilution, by physiological tests; chemical science would, I believe, fail to detect quantities of aconitine that would be still capable of producing physiological action. Dr. Dupré tells us that he chemically examined the first dilution (does he mean the first decimal or first centesimal, because there is a wide difference between them?) I take it that he means the latter, because the former would produce tingling. Mother tincture of aconite, according to the homœopathic pharmacopœia, is made from the freshly collected root or the dried root. But supposing that it were made from the dry root, which loses in drying about 70 per cent. of moisture, there would be more than double as much dry root, as if the fresh root were used. The dry root tinctures contain the activity of one part of root in ten of spirit, this is the mother tincture; to make the first decimal you take of this mother tincture one part and spirit nine parts, which makes it contain actually one-hundredth of the active or soluble parts of the root. The second decimal or first centesimal, the strength Dr. Dupré I suppose means, is made by taking one part of the first decimal and adding nine parts of alcohol: this makes it one in a thousand. Now when we consider that it takes one hundredweight of the dried root to make about an ounce of the alkaloid, I think Dr. Dupré will in fairness admit that it would be impossible to find chemically a trace of aconitine in 100 pilules, but nevertheless this alkaloid is so exceedingly powerful, that although it exists in such minutes quantities in the root that a few scrapings of the root would be more than anyone would like to eat, and it would probably entail serious consequences. With reference to atropine and strychnine, the same arguments and calculations will apply, there being a little greater proportion of alkaloid. Lastly, in reference to the profitless undertaking of convincing a true homœopath that he is wrong; who is to judge between the homœopath and the allopath? I take it that the remark applies in an inverse ratio, equally to the allopath, at any rate I hope so. I trust pride is not their principle. Further, who ever heard of an Englishman giving in after being convinced that he was right? Would Dr. Dupré?

Now if Dr. Dupré will test the action of the second decimal of the tincture of aconite, which he says according to his analysis contains not a trace of aconitine, and which he will find is too weak to cause the tingling of the tongue characteristic of aconite, I will write him a note describing to him or to any friend of his, some of the symptoms which will certainly be produced by the tincture, unless there is a personal idiosyncracy with regard to the drug. I shall be happy to send him a bottle of the tincture of the above strength, to test upon himself and his friend. Facts alone, not arguments, are the only basis for a fair judgment. He will then be in a position to say whether the second decimal tincture is capable of physiological action or not.

114, *Ebury-street*, *January* 15. ALFRED HEATH, F.L.S.

No. 31.

The Power of the Infinitesimal.*

Spectrum Analysis.

The first subject I shall bring under your notice is that of *Spectrum Analysis*. Some of the earliest experiments were made by Sir Isaac Newton more than two hundred years ago. By passing a small beam of sunlight through a circular aperture into a darkened room, and thence through a glass prism, Newton found that the apparently homogeneous white light was resolved into a band, or spectrum, of various colours.

More than a hundred years now elapsed before any important advance was made in this subject. In 1802, however, Dr. Wm. Wollaston determined to try the experiment in another way. Instead of passing the light through a circular aperture, he passed it through a narrow slit. Wollaston found, as Newton had done before, that the beam of white light was split up into a band of coloured rays, with the red ray at one end and the violet ray at the other. But in the course of his experiments Wollaston found that the band of coloured rays was divided, here and there, by dark lines.

The importance of this discovery was not fully recognised until

* This Tract is composed of extracts from the Presidential Address of Dr. Blackley, of Manchester, at the Congress of Homœopathic Practitioners, held at Bournemouth, September 18th, 1890.

another experimenter had worked at the same subject for some time. This was the celebrated optician Fraunhofer, who more or less completely mapped out these dark lines and showed their number. By viewing the spectrum through a telescope Fraunhofer found these dark lines invariably to occupy certain definite positions when the sun was at, or near, its meridian; and as a consequence he was able to number and classify them. Since Fraunhofer's time the subject has made rapid strides, and although the labours of many scientific workers have contributed to this advance, it is principally to the investigations of two German Professors (Kirchhoff and Bunsen) that we are indebted for the extensive and intimate knowledge of this subject we now possess.

The meaning and cause of the dark lines in the solar spectrum were, at first, very imperfectly understood, and it is to Professor Kirchhoff that the credit of being the first to discover the cause of their being present is due. Kirchhoff found, in experimenting with the salts of the various metals, that under the same conditions the incandescent vapour of the salt of any given metal gave rise to its own form of spectrum, but to no other. He also found that the bright lines thus artificially produced, in any given case, were identical in position with some of the dark lines of the solar spectrum, and that with the salt of the same metal these lines were always in the same position. When a flame, coloured by a salt of sodium, was placed before the slit in the spectroscope, the yellow line to which this metal gives rise was found to occupy the exact position of the double line D, or what is now known as the sodium line in the solar spectrum. Following out his experiments still further, Kirchhoff found that if the light, emanating from the burning of any given metal, was passed through the vapour of the same metal, the coloured line on its spectrum became a dark line; or, in other words, the colour became annihilated. In explanation of this, it is now said that if a coloured ray of any given degree of refrangibility is allowed to pass through the vapour of any metal having, in the light it gives off, the same degree of refrangibility, the colour becomes absorbed. In passing I may remark that this is hardly an explanation, but simply one way of stating the fact. It is undoubtedly an example of the power that one body has of destroying a phenomenon that another body with similar qualities has set up.

Mr. Norman Lockyer is also a very ardent worker in this

department of research, and it is largely owing to his experiments that we get a correct idea of the way in which the atmosphere surrounding the sun produces the dark lines in the solar spectrum. According to Mr. Lockyer, it would seem that around the sun, though at an immense distance from it, there is a cooler atmosphere from whence comes that circle of light, or corona, as it is called, which radiates into space in all directions round the hidden sphere at the time of total solar eclipse. Immediately below this cooler atmosphere, and consequently nearer the sun, there is now known to be a chromosphere, as it is termed, of incandescent hydrogen, with intensely glowing vapours of calcium and magnesium of such enormous volume and such stupendous turmoil, that red bursts of flaming hydrogen have been seen to rise within a few minutes' interval to a height of twenty-seven thousand miles!

Below this chromosphere, and yet nearer the sun, are vast quantities of metallic vapours, such as sodium, iron, zinc, copper and other elements. This is what Mr. Lockyer designates the reversing layer, from its transforming into dark lines, by absorption, portions of the sunlight shot through it. Underneath the reversing layer is the intensely heated liquid or solid matter which is known as the photosphere, and which gives off the light thus fortunately robbed of its luminous qualities in passing outwards. It is fortunate, because by the absorption of the light given off in the manner described it became possible to determine the sun's physical constitution as well as those of some of the planets and fixed stars.

In the earlier experiments that led the way to these discoveries, the attention of the scientific world was principally devoted to ascertaining and verifying the leading facts of spectroscopic analysis, without reference to the quantity of material used in making the demonstrations. But after a time attention began to be given to this phase of the question, and by degrees it was found that the quantity of incandescent metal that was being used in any given case, could be largely diminished without in any way affecting the result, so far as the production of its own coloured line on the spectrum was concerned. In the course of time it was proved that *one five-millionth* of a grain of sodium in a state of incandescence would with unerring certainty produce its specific yellow line on the spectrum. But this quantity, minute as it is, is much larger than is needed. It is now stated that *one two*

hundred millionth of a grain will give the characteristic line of this metal with as much certainty as the larger quantity.

Thus we have seen that upon the existence of the so-called reversing layer in the sun's atmosphere the possibility of proving the presence of certain metals in the sun and its surroundings entirely depends, and we now see that the demonstration of the truth of this proposition can be made with a quantity of the active agent that everyone must admit to be infinitesimal.

Photography.

We pass on now to a department of research in which the chemical action of light plays a most important part, namely, *Photography.*

For some three hundred years it has been known that the salts of silver darkened under the influence of light, but it is only in comparatively recent times that attempts were made to utilise this property of silver salts. Thos. Wedgwood (the son of Joshua Wedgwood, of Etruria) along with his friend Sir Humphrey Davy, were apparently the first observers who attempted to make any use of these salts for the purposes of photography. These attempts were, however, successful only to a limited degree.

The process invented by Daguerre was a great advance upon anything that had been done before, but even this was comparatively useless for many purposes on account of the length of exposure the plates needed in order to secure a perfectly developed image.

The principal thing which I wish to call your attention to in connection with this subject is the alteration that has taken place in the length of time necessary to expose a plate in the camera in order to produce a perfect negative. In the early days of the art it was difficult to ensure perfect stillness in living objects long enough to enable a perfect image to be obtained. This led to a continuous effort to shorten the time of exposure, by increasing the sensitiveness of the material on the plate on which the negative is taken; thus lessening the quantity or dose of light needed to produce a given effect. So great has been the success in this direction, and, if I may use the term, so infinitesimal is the quantity of light now needed to produce a perfect negative, that it is

very difficult to form a correct idea of the extremely minute portion of time it takes.

It is now said that an exposure of *one seven-hundredth part of a second* will, under proper conditions of the light, produce a perfect negative. This, I think, we may also claim as the effect of an infinitesimal quantity of the active agent.

The Microphone.

One of the most wonderful inventions of modern times is the *Microphone.* The merest accident—the snapping of a fine wire during the course of an experiment—led to the discovery, by Professor Hughes, of that marvellous magnifier of minute sounds. A watch, placed in a proper position, will yield extraordinarily loud sounds. As remarkable an experiment as any that can be made, however, is to imprison a fly, a gnat, a moth, or any other small insect in a common match box, over a hole previously cut, in one side of which a piece of muslin, or a sheet of straw paper has been stretched. Upon placing the box, so arranged upon the stand of the microphone, you will not only at once hear the previously inaudible tread of the fly, but you will hear it tramping about as though it were a horse or a bullock. It does by the sense of hear ing, in fact what the microscope does by the sense of sight, it magnifies what without its help would be absolutely indistinguishable. Another form of this instrument is the sphygmophone, by the aid of which the sound of a patient's pulse can be heard all over the room in which he is sitting. I need hardly point out that, in the matter of sound, we here descend into the region of the infinitesimal, and yet by the help of this instrument these sounds are made perfectly audible.

The Phonograph.

Wonderful as are the results obtained by the invention of the microphone, they are, in another way, completely eclipsed by the achievements of the *Phonograph.* Different individuals have, at various times, put in a claim for the inventions and improvements that have helped to make the phonograph what it now is, and no doubt some of these claims are just and proper; but if there is any one individual that has contributed in a greater degree than

any other to the improvements that have helped to make the phonograph what it is, that individual is the now celebrated Thomas Alva Edison, of New York. In one of its earliest forms his phonograph consisted of a cylinder of iron with a spiral groove cut upon it. Around this a sheet of paper was wrapped. By the aid of a screw a diaphragm with a small stylus or point attached to it was made to traverse from one end of the cylinder to the other. If the diaphragm was made to vibrate by the sound of the voice or any musical instrument, the vibration was conveyed to the style, and indentations were made in the paper around the cylinder. These, when once made, would reproduce the sounds if a point attached to the diaphragm was carried over them again by the cylinder being made to revolve. Edison after a time used tinfoil in place of the paper, but this, like the latter, was found to alter so rapidly in repeating the speeches recorded upon it, that it was soon superseded in favour of a wax cylinder. This was of a texture sufficiently yielding to permit a point to cut its way, and thus make indentations on its surface, and yet sufficiently hard to allow another point to go over it again an indefinite number of times, if the sounds recorded upon it had to be reproduced. So delicate is the adjustment of the point, and so infinitesimal is the wear and tear in reproducing the sounds, that it is said that Mr. Edison has cylinders by him that have, in each case, reproduced the speeches recorded upon them several thousand times without showing any very decided signs of being worn out.

If you examine one of these cylinders carefully, you will notice that a plain smooth line passes round the cylinder in a spiral direction before any indentations commence. This smooth line is caused by the point or style pressing very gently upon the cylinder before any sound from the voice is heard; but the moment a word is uttered loud enough to be heard, the diaphragm or disc, to which the style is attached, is set in vibration and the word is faithfully sculptured upon the cylinder.

And at this point I wish especially to call your attention to the smallness of those indentations that are visible to the naked eye. Beyond these latter, however, there are some that can only be discerned by the aid of a glass, and in some of them the amplitude of the vibration is extremely small—in fact almost infinitesimal. But however minute these are, they are, with the aid of a set of smaller or subsidiary waves, capable of reproducing the sounds

that caused them with an exactness that is almost startling the first time you hear them.

Vibrations below or above a certain number per second give no sound to the human ear. The upper limit is said to be about thirty-eight thousand per second.

If, then, we assume that one-tenth of this number (or say four thousand) of principal waves can be recorded upon a phonograph cylinder, and then consider that in some cases six times the number of the smaller waves will, of necessity, have to be recorded at the same time, it requires no stretch of the imagination to come to the conclusion that we are here dealing with infinitesimal quantities, and that these are constantly doing their work in the world.

Microbes.

We come now to a department of nature in which in its earliest, if not in all its stages of being, the infinitesimal reigns supreme. The organisms included in this department are designated by the term *Microbe*, a term that was invented in order to get rid of the difficulty of having to determine off-hand whether any given organism belonged to the animal or to the vegetable kingdom. Whether we regard it from the physiological or chemical point of view, in no department of scientific research have more important changes occurred than have been seen in this.

Liebig, in Germany, had revived the doctrine that the ferments are all nitrogenous substances—albumen, fibrine, caseine—or the liquids that embrace them—milk, blood, urine—in a state of alteration which they undergo in contact with the air. The oxygen of the air was, according to this system, the first cause of the molecular breaking up of nitrogenous substances. The molecular motions were supposed to be gradually communicated from particle to particle in the interior of the fermentable matter, which is thus resolved into new products. This theory held sway for many years. Books, memoirs, dogmatic teaching, all were favourable to the theoretic ideas of Liebig. Pasteur's investigations were, however, destined to alter all this, and to show that fermentation was in many cases due to the presence of an extremely minute living organism. Pasteur, in making known these infinitely small organisms as the cause of one of the modes of

fermentation, had discovered a third kingdom—the kingdom to which these organisms belong which, with all the prerogatives of animal life, do not require air for their existence, and that find the heat that is necessary for them in the chemical decompositions they set up around them.

The extreme minuteness of the spores of many of these microbes furnished, as I have said above, one of the greatest difficulties with which the investigation of their action was surrounded. More than one of the advocates of spontaneous generation had demanded that if these organisms infested the atmosphere to the extent they were supposed to do they ought to be able to be collected and weighed. Few even of the scientific men of that day seemed to have any exact notion of the weight of these germs, and even now, when so much has been done in investigating the life history of some of the microbes, it is almost impossible to form a correct idea of the weight of the germ in its earliest state.

I have myself succeeded in weighing some of the smaller organisms. The pollen grains of the grasses vary in weight from one four-millionth to one ten-millionth of a grain. Some of those belonging to other natural orders vary from one thirty-millionth to one forty-millionth of a grain. But the spores of some of the cryptogams are exceedingly minute. In one of the fungi that resembles the edible mushroom, the spores weighed rather less than *one five hundred millionth of a grain.* These are probably nearly twenty times as heavy as the spores of the *penicillium glaucum*, which I have never yet succeeded in weighing accurately. But these latter are perfect leviathans compared with the spores of the microbes.

And here at this point I must call your attention to the series of elaborate and extremely interesting researches on the life history of the monads, made by my esteemed friend Dr. Drysdale, in conjunction with his friend and co-worker, Dr. Dallinger.* In these researches animal matter was macerated in water until monads had been generated and had grown to the fully developed form. One of these was kept under continuous observation until it had gone through all its changes and had discharged its brood

---

* *Researches on the Life History of the Monads.* By the Rev. W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D., F.R.M.S. Reprints from the *Monthly Microscopical Journal.*

of young monads or spores. So minute were these that in speaking of them Dr. Dallinger and Dr. Drysdale say: "It became now a matter of great interest to study the future of these infinitesimal spores. With $\frac{1}{25}$ (of an inch objective) the most accurate observer could not have discovered their presence if he had not previously seen them with the $\frac{1}{50}$." This latter, however, with an A eyepiece gave a magnifying power of *two thousand five hundred diameters*, and yet with this enormous power these spores appeared as a mere nebula under the object glass.

It is exceedingly difficult to give even an approximative idea of the size of such spores, and quite impossible to give anything like a correct estimate of their weight, but from what I have said above, of the weight of other spores that are immensely larger, it will be seen that those of the monads referred to must be infinitesimal in the highest degree. It is, moreover, certain that, if some hundreds of millions of these organisms were present in the blood stream, and could be separated at will, this number would not affect the best balance that has ever been constructed.

It is now admitted by many pathologists that certain microbes set up disease when they obtain an entrance into the blood stream, but it has not yet been determined, in all cases, whether the spore, in its earliest form, or the fully-developed microbe has the greatest share in producing the disturbance that they cause. It is, however, tolerably certain that the number I have named above could not be present in the blood without setting up a considerable amount of disturbance, although the actual quantity, as far as weight is concerned, would be infinitesimal in a high degree.

Although the opinion has been contested by some observers it has been stated by Laveran and Richard—two military surgeons—that a microbe is found in the blood of patients affected with intermittent fever. Richard says that the multiplication of these bodies must be extremely rapid, as "they are not found in the intervals of the attacks. As the attack approaches they appear in increasing numbers, and their maximum corresponds with the beginning of the rise in temperature; from that moment they begin to perish, since the heat of fever is fatal to them and completely checks their development. They produce fever, the fever kills them, and then subsides: when the fever heat departs they multiply again, excite fever, and so on."

Darwin's Experiments on Drosera.

In a paper read by me some years ago at one of our Congresses, amongst other matters I alluded to the experiments of the late Mr. Darwin on Insectivorous plants. At that time we had very little of the details of the experiments given; but since the publication of his life and letters by his son we have all the particulars by which he arrived at his conclusions given. I propose briefly to notice these and to call your attention to the results of his careful investigations.

In his autobiography, Darwin says: "In the summer of 1860 I was idling and resting near Hartfield, where two species of *Drosera* abound; and I noticed that numerous insects had been entrapped by the leaves. I carried home some plants, and, on giving them insects, saw the movements of the tentacles, and this made me think it probable that the insects were caught for some special purpose. Fortunately a crucial test occurred to me, that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density, and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation."

Darwin followed up these investigations and gradually brought out results that greatly surprised and apparently troubled him. In writing to his friend Dr. Gray about some of his earlier experiments, Darwin says:—"I have been infinitely amused by working at the *Drosera;* the movements are really curious, and the manner in which the leaves detect certain nitrogenous compounds is marvellous. You will laugh; but it is, at present, my full belief (after endless experiments) that they detect (and move in consequence of) the 1-2880th part of a grain of *nitrate of ammonia*."

Later on, in writing to another friend, he says: "I had measured the quantity of weak solution and I counted the glands which absorbed the *ammonia* and were plainly affected; the result convinced me that each gland could not have absorbed more than 1-64,000th or 1-65,000th of a grain. I have tried numbers of other experiments all pointing to the same result. Some experiments lead me to believe that very sensitive leaves are acted upon by much smaller doses."

Again, in writing a little later on in the same year to his friend Sir J. D. Hooker, he says: "I have been working like a madman

at *Drosera*. Here is a fact for you, which is as certain as you stand where you are, though you won't believe it, that a bit of hair 1-78,oooth of a grain in weight, placed on a gland, will cause *one* of the gland-bearing hairs of *Drosera* to curve inwards, and will alter the condition of every cell in the foot-stalk of the gland."

Thus Darwin went on from step to step carefully trying the effect of smaller and smaller quantities of *ammonia salts*. In a letter to Dr. Burdon Sanderson, he says: "I must tell you my final result of which I am sure [as to] the sensitiveness of the *Drosera*. I made a solution of one part of *phosphate of ammonia* by weight to 218,750 of water; of this solution I gave so much that a leaf got only 1-1,552,oooth of a grain; this being absorbed by the glands sufficed to cause the tentacles bearing the glands to bend through an angle of 180°." Again, in writing to his friend Asa Gray about the smallness of the dose of *phosphate of ammonia* that would move the tentacles, he says: "No human being will believe what I shall publish about the smallness of the doses of *phosphate of ammonia* which act."

Having been told by his son that Professor Donders had stated to him that so small a dose as the one-millionth of a grain of *atropine* would act upon the eye perceptibly, Darwin wrote to Professor Donders as follows:—"Now will you be so kind, whenever you can find a little leisure, to tell me whether you yourself have observed this fact or believe it on good authority. . . . The reason why I am so anxious on this head is that it gives some support to certain facts repeatedly observed by me with respect to the action of *ammonia* on *Drosera*. The 1-4,000,oooth of a grain absorbed by a gland clearly makes the tentacle which bears the gland become inflected; and I am fully convinced that 1-20,000,oooth of a grain of the crystallised salt (*i.e.*, containing about one-third of its weight of water of crystallisation) does the same. Now I am quite unhappy at the thought of having to publish such a statement. It will be of great value to me to be able to give any analogous facts in support."

Professor Donders subsequently corroborated his statement, and Darwin fully confirmed the conclusions he had arrived at. But he even went beyond his estimate of 1-20,000,oooth part of a grain, and calculated that if we deduct the amount of the water of crystallisation from the dose of the salt administered to each gland, the quantity of active material would be less than 1-30,000,oooth of a grain.

In his concluding remarks on this part of the subject in his work *On Insectivorous Plants* Darwin says:—"There is nothing remarkable in the fact of one twenty-millionth part of a grain of the phosphate, dissolved in about two million times its weight of water, being absorbed by a gland. All physiologists admit that the roots of plants absorb salts of *ammonia* brought to them by the rain; and fourteen gallons of rain water contain a grain of ammonia, therefore only a little more than twice as much as the weakest solution employed by me." He then goes on to say that the wonderful fact is that 1-20,000,000th part of a grain (including less than one-thirty millionth of efficient matter) should cause the basal part of a gland to bend through an angle of above 180°.

Though the *Drosera* is apparently not endowed with a true nervous system, the action of the *ammonia* seems to have been the same as if a nerve tissue had been present in the gland. Darwin came to the conclusion that a continuous line of protoplasm served the same purpose and transmitted motor power to the base of the foot-stalk of the gland. However this may be, the experiments furnish an excellent example of the action of infinitesimal quantities even on an organism that is low down in the scale of creation.

The Author's Investigations on Pollen.

With one exception the facts I have so far brought under your notice lie somewhat beyond the pale of medicine proper. I must now, in conclusion, notice one example of the power that very minute quantities of *a disease-producing agent that does not belong to the zymotic* class* have in bringing on disease; and in doing this I must be permitted to refer to some of my own investigations.

In the second edition of my work on hay-fever I show that the 1-40,000th of a grain of pollen inhaled in each twenty-four hours will commence the malady, and that 1-3,427ths of a grain will suffice to keep it up at its highest point of intensity: but one of my reviewers in 1880 expressed great doubt about the accuracy of my conclusions, and intimated that these quantities could not represent the quantity inhaled under some circumstances. The experi-

* A term applied to causes of disease supposed to act after the manner of ferments.

ments had been very carefully made, and as nearly as possible represented the average quantity of pollen a patient would inhale in pursuing his daily avocations, unless his duties compelled him to be in the midst of hay-grass during the whole period of its flowering.

A case that came under my notice a little time ago caused me to determine to ascertain what was the largest quantity of pollen a patient would inhale if kept in the midst of flowering grass during the whole of the hay season, and for reasons that I shall presently give this case was much more interesting than an ordinary one. The patient, a lady, residing in the west of England, was a terrible sufferer from hay-asthma, and her husband came over to consult me on her account. The previous season (1886) had been spent in the island of Heligoland, where the patient was tolerably well during the whole time she remained, but from the description given to me I should imagine this was one of the worst cases of hay-asthma I had ever been consulted upon. Her home was in the very midst of land used for the growth of hay grass, so that here in the hay season we should have a large quantity of pollen generated. The object of the consultation was not so much that of treatment by medicine, as to see if it would be possible to prevent the pollen gaining access to the apartment in which the patient was sitting, and thus to avoid the necessity of her leaving home at a given time each year, whatever, in other respects, her state of health might be. I at once gave the opinion that this could easily be done if a proper apparatus was used, and gave a sketch of one that I thought suitable. This consisted of a square tube of wood, ten inches by ten, in which were placed three muslin screens (double layers) moistened with glycerine and carbolic acid. This the gentleman had fitted up, and at the same time had an air propeller fixed so that a current of air could be driven through the tube as often as desired.

For two years the experiments were carried out at Ramsgate, and were fairly successful, but this year they have been carried out at their own home in Gloucestershire. An extra room has been added to their house, and a small gas engine has been put down to drive the air propeller. A trial of the apparatus has been fairly made this year and appears to have been very successful. The husband of the patient writes me and says :—"I keep the engine going twelve to fourteen hours a day, thus keeping the room well supplied with fresh air. I think it due to you to inform you

what the result has been, and I know you will be pleased to hear that though in the midst of grass and hay making she (my wife) has had no symptom at all of hay fever."

The question with me in this case was what was the largest quantity of pollen the patient could have inhaled each day if she had been left in her usual condition when at home during the hay season? As obviously I could not go to Gloucestershire to follow out experiments there, I did the next best thing by selecting a locality surrounded by land devoted to the growth of hay-grass, and in this respect closely resembling the one in which the patient resides. Here a fresh set of experiments was carried on. The apparatus I used was a very simple one and consisted of a flattened glass tube, with a row of microscopic cells top and bottom. Each cell was moistened with glycerine, and by inhaling through this tube every pollen grain was deposited in one or other of the cells; and the pollen usually was all deposited before the tenth pair of cells was reached—the largest number being of course detained in the first pair.

The experiments were commenced last year, when the grass began to be fairly in flower. One thousand inspirations through the tube were made at each experiment, occupying about an hour each, whilst at the same times the eyes and nostrils were protected. On two occasions breathing was carried on (after the inhalations through the tube) for the same length of time, but without the protection to the eyes and nostrils, in order to see what symptoms would be developed. The last occasion was when the grass was fully in flower, and the symptoms were so severe I was glad to conclude the experiment before the proper time had expired.

The minimum quantity obtained at the commencement of the experiments was 1-240,000th of a grain, and the maximum was 1-30,000th of a grain. So that the largest quantity the patient could have inhaled, in a day of ten hours, would have been one twenty-four thousandth of a grain as a minimum, and one three thousandth as a maximum. From careful and oft-repeated experiments, I am certain that so small a quantity as the 1-100,000th of a grain of pollen will give rise to very perceptible symptoms if this is inhaled within a given time. In fact, this capacity for acting in so small a quantity lies partly in the fact that every pollen grain when it comes in contact with the mucous membrane is detached and has its own sphere of action, unimpeded by anything immediately in contact with it.

In the case I have given above we have a double testimony, viz., the severe suffering caused by the presence of an infinitesimal quantity of the exciting cause of the disease and the perfect freedom from this suffering when this infinitesimal quantity was taken out of the air the patient breathed. But this experiment has an additional interest to me. It has for many years been a favourite idea with me that it would be quite possible to free the air from the immense number of infinitesimal germs that float in it. Here we have a proof that this can be accomplished, and I cannot help thinking that this method of obtaining pure air will some day become a valuable help in the treatment of some diseases.

The following Tracts have appeared, and may be obtained on application to the Secretary.

1.—Why should the Friends of Homœopathy form a League? 1d.
2.—What is Homœopathy? 1d.
3.—Origin of Homœopathy. 1d.
4.—Hahnemann's achievements in Medicine and the Allied Sciences. 1d.
5.—Statistics of Homœopathy. 1d.
6.— Persecutions of Homœopathy. 2d.
7.—Testimony of Opponents in favour of Homœopathy. 1d.
8.—Influence of Homœopathy on Ordinary Practice. 1d.
9.—Allopathy Judged by its Professors. 1d.
10.—Eminent Physicians who have embraced Homœopathy. 1d.
11.—Official Trials of Homœopathy. 2d.
12.—Dangers of Modern Medicine. 1d.
13.—From Empiricism to Homœopathy. 1d.
14.—The Royal College of Physicians of London and Homœopathy. 1d.
15.—Explanation of Dr. Lauder Brunton. 2d.
16.—How they were converted. 2d.
17.—How to study Homœopathy. 1d.
18.—Allopathic Misconceptions of Homœopathy. 1d.
19.—Gains of Medical Liberty in Fifty Years. 1d.
20.—"The Times" on Homœopathy. 1d.
21.—British Homœopathic Hospitals. 1d.
22.—The Small Doses of Homœopathy. 1d.
23.—Homœopathy in the Old School. 1d.
24.—Economy of Homœopathy. 1d.
25.—Progress of Homœopathy. 1d.
26.—Allopathic and Homœopathic Treatment Contrasted. 1d.
27.—The Common Sense of Homœopathy. 1d.
28.—What Homœopathy has saved us from. 1d.
29.—The Reign of Law in Medicine. Price 1d.
30.—How the Powers of Drugs were Determined. 1d.

The first 18 *Tracts are published in one volume, with index, price* 1*s.* 6*d. A new volume commences with No.* 19.

PUBLISHED FOR THE HOMŒOPATHIC LEAGUE BY
J. BALE & SONS, 87-89, GREAT TITCHFIELD ST., LONDON, W.
Price 1d.; per doz., 8d.; per hundred, 4s. 6d.

Information respecting the League may be obtained from the Hon. Secretary, Mr. E. H. LAURIE, 16, Blandford Square, London, N.W.

THE PUBLIC ASPECTS OF MEDICINE.

British Medical Association,

FIFTY-EIGHTH ANNUAL MEETING, BIRMINGHAM, 1890.

ADDRESS IN MEDICINE FOR 1890.

THE PUBLIC ASPECTS OF MEDICINE.

BY

SIR WALTER FOSTER, M.D., M.P.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, LONDON;
MEMBER OF THE GENERAL MEDICAL COUNCIL;
SENIOR PROFESSOR OF MEDICINE AT QUEEN'S COLLEGE; PHYSICIAN TO THE GENERAL HOSPITAL, BIRMINGHAM; CONSULTING PHYSICIAN TO THE KIDDERMINSTER INFIRMARY AND WEST BROMWICH HOSPITAL, ETC., ETC.

Reprinted from the BRITISH MEDICAL JOURNAL, August 2, 1890.

Birmingham:
CORNISH BROTHERS,
NEW STREET.

CONTENTS.

THE "BRITISH MEDICAL JOURNAL"

ON THE

ADDRESS IN MEDICINE FOR 1890.[1]

Sir Walter Foster, in his admirable and eloquent Address in Medicine, traces the progress and mode of advancement of our art during the time of his own active professional life, and curiously enough it happens that although our Association is of greater age than the lecturer, his clasification of the advances Medicine has made almost exactly covers the period of its existence. Sir Walter speaks of the strong clinical traditions of his own (the Dublin) school, but it was in the same year in which Graves, its father, delivered his second introductory lecture on clinical instruction that the Provincial Medical and Surgical Association held its second annual meeting in the very town (Birmingham) where the British Medical Association holds its fifty-eighth annual meeting to-day. At that second annual meeting Dr. Conolly spoke, in his introductory address, of the approaching publication of a third edition of Dr. Forbes's translation of Laennec's *Treatise*, a work which, he truly said, "has done more to promote an exact acquaintance with diseases of the lungs and the heart, than any book which has appeared." He then went on to proclaim the indebtedness of all his contemporaries to him, whom he called "the great auscultator."

---

[1] This leading article appeared in *The British Medical Journal* of August 2nd, 1890.

It is a happy coincidence which finds Conolly's accomplished successor of to-day, after a lapse of fifty-six years, occupying much of his address with the work and praises of another Frenchman, greater perhaps than Laennec, whose important discoveries will probably have as wide an influence on the prevention of disease as Laennec's had on its diagnosis.

It is curious in looking back on this period, as Sir Walter Foster invites us to do, to note the advance of true knowledge, yet ever partial and intermingled with error. First we see the clinicians, who held, in the words of Graves, "that practical medicine" (that is, the clinical recognition and therapeutics of disease) "was the most important of all branches of professional knowledge." Some of Graves's contemporaries even declared "that morbid anatomy was only instructive after the death of the patient, and even then not infrequently calculated rather to mislead than to advance the interests of practical medicine;" and the great teacher himself, though enlightened and liberal beyond his fellows, spoke in terms of strong disapprobation of students "wasting half the time which should have been spent in hospitals and by the sick bed in wandering through the fields on botanical expeditions, or working in the laboratory engaged in the solution of some unimportant problem."

It was natural that physicians should grow dissatisfied with auscultatory sounds and therapeutic theories, and so, in looking beyond, gradually develop into mere morbid anatomists, who, "disheartened by the ravages of unchecked disease," and recognising in histological alterations of structure changes which no medicines could affect, became what Sir Walter Foster calls cynical Hippocratists, who were content "to confess their impotence to grapple with developed disease and cut it short." Not that these men were totally wrong: theirs was a great advance on the therapeutic theories of their predecessors, who saw in drugs the cure-all of every disease.

Theirs was the common fault of seeing and gladly accepting half-truths, of not looking deep enough, of forgetting the warning of the greatest of the clinicians when he bade them remember "that the first alteration in the texture of a part was not the cause, but the consequence, of disease."

This hopelessly passive condition of mind in the morbid anatomists led to its natural cure by the impatience it provoked in more aggressive thinkers, who, not content with histological alterations as ultimate explanations, sought beyond them for a further causation. The outcome of this reaction was, as Sir Walter Foster shows us, those bacteriological inquiries and discoveries which have immortalised Pasteur, Lister, Koch, and others who now occupy in our generation the places which Harvey, Hunter, Jenner, and Laennec occupied in the days of the past—places which had hitherto seemed unfillable by the pigmies of our later days.

It is interesting, in the light of these historical remarks of Sir Walter Foster, to notice the interdependence of scientific workers of various ages. It would be as foolish in the present race of bacteriologists to despise their immediate predecessors, the morbid anatomists, as it would have been in the latter to have derided the clinicians of the preceding generation, or in these again to have laughed at their forerunners, the nosologists. In truth, the existence of one generation is as necessary to the production of its successors in science as in Nature. For finite man it is only possible to commence at the outside and work towards the centre. The nosologists began the great labour by developing "diseases" out of "disease." The clinicians clearly defined diseases through their signs and symptoms; the morbid anatomists traced them to definite anatomical lesions; and now new workers are further tracing out these lesions to their ultimate causation; so that each generation has added something to the total structure, even if each has had to take something of his predecessors' work away.

Every son has built on the foundation of his father, and so each of us ought to feel himself connected with our predecessors by a natural piety, which forbids in any one of us aught but loyal and respectful recognition of the labours of the dead.

Hence it is that, apart from its eloquence, Sir Walter Foster's address has such high value. It prevents us from ignoring the work of our great predecessors, but at the same time it shows us that the future is ours; and above all, it tells us that amid all the theories of the schools, amid the jangling disputations of controversialists, truth and knowledge are ever advancing. Science—and especially medical science—is like the flowing tide. Judged by the advance and retreat of single waves hardly any progress seems to be made. Indeed, at times the tide seems to ebb rather than to come on. It is only when a comprehensive view of the beach is taken that we see how far the waters have already encroached. Rocks and stones which but a few moments ago stood high and dry on the sand are now overwhelmed by the waves. Sand paths round headlands, but a short time since safe and inviting, are now masses of boiling foam.

And so with Medicine. Among new theories, advanced, disputed, withdrawn, and perhaps advanced again, one too often seems to see signs of retrogression rather than of advancement. It is only to one standing on vantage ground and surveying the history of our art through a prolonged period that real progress is apparent. It is on such ground Sir Walter Foster takes his place. But beyond the especial historical uses of this portion of the address, it has a higher mission, since it holds out hope to the honest and earnest worker. It tells him that, since Medicine is an advancing science, each day must add to its possible attainments and triumphs. If it is to the future, not to the past, we must look for perfection, it is the youngest amongst us who has the most to hope for.

Theology and history may turn their gaze regretfully backward towards a vanished golden age ; Medicine gazes hopefully onward to the days which are to come ; in them she places her highest expectations. And so the wise physician is ever an optimist. Dr. Conolly, in 1834, saw in his own age one distinguished from all preceding ones by a general thirst for knowledge," yet only the mere herald and forerunner of one still better, of which he prophesied "that its proud distinction should be to have found great masses of mankind deeply ignorant of much that concerned their health and happiness, and to have left them better protected against illness and misery ; to have found them debased, and to have left them advancing ; to have found them sunk in insensibility to their moral and intellectual power, and to have left them awakened to a sense of the exertion and duties to which God and Nature had called them."

In like manner to-day Sir Walter Foster paints us a brilliant future. In an age when enteric fever is in some towns endemic, he pictures the time when medicine, through the discoveries of to-day and to-morrow, shall prevent instead of seeking to cure disease. At a time when social rank professes to look down on medicine, when army surgeons are denied any recognition, when Poor-law medical officers are paid a pittance and refused a pension, he paints the day, and calls it no distant one either, "when the State shall awaken to the value of such services, and recognise in the trusty dispenser of a nation's charity, or the wise saviour of a city's health, servants of the State more worthy of its honours than the successful soldier or the astute diplomatist."

In the days of indifferentism, of toadyism, of "junkerism," of red-tapeism, and of all the other dozen and one "isms" which, like leeches, sap the life-blood of our nation and profession, such words as these would only seem the impotent cry of a hopeless desire, were it not for men like their speaker,

who, putting philanthropy before party, and duty before self, spend their lives in the unpaid service of humanity, and in the cause of their defenceless brothers ; were it not also that he and such as he are but the spokesmen of this influential Association whose first voice, as its latest, was raised on behalf of suffering and neglected mankind.

To-day Sir Walter Foster's noble words ring out like a trumpet, calling on each member of our profession to prepare for the battle—that battle for which the meet preparation is the service of the poor ; whose cause is the cry of the downtrodden, whose combatants are on the one side the giants disease, neglect, officialism, and injustice, on the other right and knowledge—which may be long, but must inevitably end in victory ; whose reward is the praises of the poor ; whose result must be the welfare of the human race.

ADDRESS IN MEDICINE.

Delivered at Birmingham on the occasion of the Fifty-eighth Annual Meeting of the British Medical Association.

THE PUBLIC ASPECTS OF MEDICINE.

The Experimental Method.—Its Achievements in Bacteriology and in Physiological and Pathological Chemistry.—The Influence its Results have had on Public Opinion and on Legislation.—Legislative Recognition of Continuous Responsibility in matters of Public Health.—The Sanitary Achievements of the Past and the Needs of the Future.

Mr. President,—In the first place it is my duty to acknowledge the honour conferred on me by the Council of the Association and by my colleagues in Birmingham, in appointing me to deliver the Address in Medicine. I value the distinction all the more, because it places me in a position of responsibility under your presidency, and so recalls to me very vividly the pleasant memory that, some thirty years ago, I did my first medical work in this town as your junior colleague on the staff of the Queen's Hospital. I can recall with grateful remembrance the many conferences we had in those days on the problems that confronted us in our daily work, and when I do so I am startled by the marvellous change that has occurred in the mental attitude with which the profession now regards disease. Like yourself, Mr. President, a student of the Dublin School of Medicine—a school then essentially clinical in its traditions—I had come to Birmingham to breathe a new atmosphere of modern thought and scientific enterprise. The school I had left was the one in which authority held sway with unbroken strength latest in the United Kingdom, and the school which I joined was in a town not celebrated at any time for a

blind reverence for authority, and never much influenced by ancient traditions. It was a fresh and bracing intellectual change of air. The characteristics of the place and the time were a healthy scientific scepticism—a dogma-destroying doubt, before which cherished credulities were crumbling away, and religious and social faiths were questioned and analysed with a new spirit and a new freedom.

Well, in those days, in the struggle between the old principle of authority and the modern spirit of doubt, it was beginning to go hard with the metaphysical battle cries of the old school of medicine. The *vis medicatrix naturæ*, which had been a popular fetish, and the change of type theory of disease, which had all the consoling virtues of extreme unction for the passing generation, were rudely questioned by the younger men as final explanations, and were being generally realised at their true values as temporary hypotheses based on partial truths. They had served our predecessors, groping their way through metaphysical mists, but they had now become impediments to the onward march of exact thought. The new school,

> Not clinging to some ancient saw,
> Not mastered by some modern term,

confined its attention to the direct study of the actual physical phenomena of disordered health. Imbued with the traditions of the Dublin School, one could look back lingeringly on the clinical conquests of the old masters, before joining the modern leaders in the new crusade of experimental medicine. It was a veritable revolution, and with it came a natural reaction against the empiricism of the older time, which resulted in a nineteenth century revival of the Hippocratic school. The study of the natural history of disease was once more exalted by some to the highest place, and the function of therapeutics lowered to the so-called "expectant method." Fortunately, this modern revival of what had been called of old a mere meditation upon death (*θανάτου μελέτην*) had in its extreme form but a short success. There was not enough inspiration in a gospel that taught its disciples, that a warm bed was the treatment for pneumonia, and that blankets and mint water were the remedies for the most painful of acute maladies. I said fortunately, because if this expectant school had long maintained its influence, it would have been disastrous to our art, which must always rest on exact diagnosis as the basis of honest treatment.

It is hardly too much to say that diagnosis itself would have in time become a superfluity to practitioners, who regarded a warm bed as the simple and sole treatment for a pneumonia, a pleurisy, or a rheumatism. In our ranks, as in mankind generally, the aggressive courage that volunteers to advance on the terrors of the unknown, is more common than the passive fortitude that stolidly endures evil. As Trousseau said, "It is better to walk in darkness than to stand still." The cynical confession by these modern Hippocratists of their impotence to grapple with developed disease and cut short its course, roused the impatience with which disciples of the aggressive school have always refused the passive function of mere observation. The external forms of diseases and the laws which regulate their courses were already well, if not perfectly, known, and so students were stirred more and more in the face of teachings of therapeutic powerlessness to search with renewed energy into the causes which produced disordered health. The study of medicine became more and more the study of experimental Pathology, not the morbid anatomy that generated despair by its revelations of the ravages of unchecked disease, but that modern Pathology, which, armed with instruments of precision and methods of the minutest delicacy, has sought out the very beginnings of disease, and discovered new possibilities of final triumph. Thus, side by side with the teachings of Gull and his followers, grew the new school of experimental medicine. The time, too, was propitious. The physical sciences had reached a stage of their growth, when medicine could borrow their methods, and relying no longer solely on observation, could apply experiment and comparison to the solution of her problems. "To search out and study the secrets of nature by way of experiment," the great Harveian principle, was to bear fruit. Villemin, in 1865, startled the world by showing the inoculability of tubercle, and Sanderson repeated and confirmed his experiments. The work has gone on ever since till now, thanks to the brilliant labours of Koch, we can identify and isolate the bacillus of tubercle. The micro-organism has been cultivated and studied, till the evidence has become complete enough to satisfy most of the sceptics, that a tubercle is not merely a neoplasm of definite histological structure, but a neoplasm containing within it a specific bacillus. It would be difficult to exaggerate the value of this great conquest of experiment, which is dissipating day by day the mystery that has through the ages surrounded consumption. Like other similar discoveries hereafter to be referred to, its chief value at present is in the explanation it

gives of the communicability of the disease, the conditions which favour its development, and the means to which we look for prevention, if not for cure.

Long before this, we had, in seeking an explanation of many infectious maladies, assumed that some sort of fermentative process went on in the system, and so we called them zymotic diseases. When the labours of Schwann had demonstrated that the act of fermentation was intimately connected with the multiplication of living yeast cells and the result of their life, a beginning was made for the germ theory of disease. The study of putrefactive processes and their intimate association with micro-organisms distributed in air, earth, and water was the next step. The doctrine of a contagium vivum as the cause of each specific disease received fresh support, and the vitalistic theory of fermentation, elaborately supported by Pasteur, led to the greatest of modern surgical triumphs in the treatment of wounds—a triumph which has added glory to British surgery and immortalised the name of our associate, Joseph Lister. The fermentative and putrefactive processes were, under the growing sense of their importance, investigated with wonderful patience and remarkable skill alike by those who held the vitalistic theory of their causation and those who defended chemical views. In the end the great chemical champion, Liebig, was dislodged by the experiments of Naegeli from a series of positions which he had defended with the greatest skill, and the presence of micro-organisms was generally accepted as essential to fermentation and putrefaction. The writings of Henle, nearly fifty years before, had foreshadowed the connection of infective diseases with micro-organisms, and a distinguished member of this Association, Dr. William Budd, of Bristol, had, in 1849, declared his belief that cholera and typhoid fever depended on living organisms. He, like many an ardent student, longed for and foresaw the day, when, in connection with zymotic diseases, the initial phenomenon of the morbid series would be isolated and defined, as had been done in the case of parasitic diseases like tinea or scabies. That day was now at hand. The discovery of the bacillus of anthrax by Pollender, and the subsequent elucidation of its life-history by Davaine, Pasteur, Koch, and others, who proved it to be the actual cause of the malady, opened a new field of study. New and ingenious methods of investigation, new arts of studying the life-history and morphology of bacteria by cultivating them in nutrient media were devised; and inoculation experiments on

animals revealed new and startling facts as to the nature and modifications of the virulence of micro-organisms, which led to the discovery of means by which they could be weakened at will and made protective against more potent forms.

Modern Medicine had been reproached by a great clinician, as tending to lose itself in the study of the infinitely little, but the infinitely little had now a noble revenge in creating a scientific basis for preventive medicine. Bacteriology became a special branch of science, armed with its own methods, worked by its own students—a special army corps equipped with search lights for exploring the positions and studying the tactics of the common enemy, disease. Relapsing fever yielded its secret cause, in the spirillum of Obermeier in 1872, while researches on the diseases of animals discovered bacilli as the causal agents of many of their ailments. The chicken cholera investigations of Pasteur were probably the most fruitful, for they first led him to the discovery of the method of attenuating the virus by the action of the air on his cultures of the microbe. The virulence was lessened while the morphological characters were retained. A strong or a harmless virus could be produced at will, and at a certain strength it formed a vaccine preventive of the original malady. Jenner's discovery had found a parallel, and the method of inoculation had gained extended application.

Similar methods applied to the study of the bacillus of anthrax led Dr. Greenfield, in this country, and Pasteur, in France, to the conclusion that its virus could be also attenuated by cultivation, and that inoculation of the attenuated form, while producing mild splenic fever, was protective against future attacks of the disease. In 1881 Pasteur publicly demonstrated the efficacy of these protective inoculations, and now every year hundreds of thousands of animals are successfully vaccinated against anthrax and saved infinite suffering, while man is less exposed to woolsorter's disease. There is yet another example, in a disorder affecting pigs, called in France "rouget," and in Ireland "red soldier," from the red patches that appear on the skin in fatal cases. This affection depends on a bacillus, and in studying it Pasteur devised a new method of attenuation. He inoculated rabbits with the virus, and discovered the remarkable fact that in passing through rabbits the energy of the virus increased for them, while it diminished for the pig; till at a certain point it became a protective vaccine for the swine. The genius of a French chemist thus opened out to our view, in these and other experiments, the far-

reaching possibilities of preventing disease which the great Englishman began in the discovery of vaccination. The discovery of our day, however, by isolating the initial phenomenon of the morbid series, and showing us how we can modify and control its virulence at will, arms us with a knowledge that promises far wider results for prevention and cure.

In the dark ages chemistry had been nursed and nourished by medicine, and in our time the daughter science, by the application of experiment, had won its brilliant position. It was therefore a fitting return that medicine should owe to a great modern chemist the scientific methods which have resulted in the grandest findings of our time. In Pasteur's hands experiment and comparison have so widely extended the domain of pathology as regards man and animals, that we begin to realise the new kingdom of comparative pathology which the genius of Hunter foresaw and described.

> 'Tis time
> New hopes should animate the world, new light
> Should dawn from new revealings.

Pasteur's most difficult task, however, was the discovery of a prophylactic for hydrophobia. Working on the same lines, although he did not succeed in isolating the special microbe of rabies, he nevertheless found a means of modifying the virus, of shortening its incubative period, and inventing a system of inoculation which is protective against the dog bite. Hydrophobia is supposed by Pasteur to be due to a living contagium capable of multiplication and to a secondary poison or ferment, which is produced by the growth of the micro-organism in the system. Now there is a law widely applicable to most zymotic or pathogenic organisms, that the substances they produce become in the end destructive of their own development. The most familiar illustration is vinous fermentation, in which the alcohol formed checks the growth of the yeast fungus. Similar facts have been observed in connection with several pathogenic microbes. In such cases the saturation of the system with the products of the microbe stops its multiplication, and in time renders it inert. If this theory be well founded, the effect of repeated inoculations of the chemical product is to introduce into the body, in the form of attenuated virus, enough of the product to prevent the fresh development of the original micro-organism. As I have said, this original micro-organism has not been isolated in the case of rabies. There is consequently a gap in our knowledge of the *modus*

operandi of the virus in that instance, which leaves us in much the same position as we are with respect to vaccination and small-pox. Admitting this freely, we can still agree with a great authority in saying: "There is no stronger example of the power of the experimental method applied to medical matters than this one of the prevention of a malady, the absolute virus of which is still obscure."[2] Looking closely at this theory, which may explain Pasteur's success, we get a side light of startling suggestiveness as to the truth at the bottom of the old theory of the *vis medicatrix naturæ*, just as in the modifications in the intensity of the virus produced by cultivations, we may recognise the truth underlying the change of type theory of disease.

In the most malignant infective or febrile disorders there is often such profound constitutional disturbance that some poison or ferment is suspected to be generated in the blood. This chemical side of the pathological inquiry has yet to be worked out, but already chemists have isolated from organic substances undergoing putrefaction complex compounds which possess most virulent properties. In some cases of septic disease these toxic products, there is reason to believe, are generated by the metabolism taking place between the bacteria and the tissues. Some of the latest researches lead us to think that a given bacillus will always produce identical metabolic products, and that some of these are the specific results of the particular bacterium, and cause the graver symptoms as, for instance, in diphtheria. It is not my object in these cursory references to bacteriological discovery to do more than indicate the direction of modern advance. Much as one would like to dwell on the experimental results obtained in other diseases, such as cholera, pneumonia, scarlatina, typhoid, and septic and suppurative conditions, I am obliged, by the limits of my time and the nature of my object, to refrain.

The position at which we have arrived may be briefly stated as follows. In some few diseases, such as anthrax and relapsing fever, we know a specific micro-organism to be the contagium. In a second group, such as tubercle and cholera, the evidence is nearly complete; while in a third group the position of the micro-organism as the cause of each malady is still *sub judice*. In scarlatina, for example, the existence of a specific germ has been warmly discussed by Klein and Crookshank. Edington claims to have found a bacillus, Klein and others describe a specific strepto-

[2] Croonian Lecture on Preventive Inoculation, by M. Roux, JOURNAL, June 8th, 1889.

coccus. Fränkel and Freudenberg admit the streptococcus, but say it is not special to scarlatina, but identical with the streptococcus pyogenes, which is common to many septic and purulent conditions. So the matter rests, and we may leave it with confidence in the hands of such workers, to whom I ought to add the name of Dr. Crooke, of this town, the able Secretary of the Pathological Section, who has the honour to have been the first to observe and describe a streptococcus in scarlatina.

The lesson that these results teach us is that experiment and comparison have vastly changed our notions of disease, by substituting actual demonstrations of morbid processes for vague speculations. We no longer refer an epidemic to the anger of the gods, or to some intangible emanation, but we find its cause with the microscope and the culture flask. We identify and isolate the microbe, and that done, or even before it is done, we learn by experiments to modify and master its effects. "Shutting out fear with all the strength of hope," experimental medicine aims at discovering and controlling the starting-point of each infectious malady. In every instance in which that is accomplished the arts of prevention and cure hasten forward with hurrying feet, but with by no means equal steps. Bacteriology gives, in the first place, the strongest impulse to preventive medicine, by defining the cause, and in many cases giving the power to control and modify at will the initial phenomenon of each morbid series. With these revelations of the nature of pathological processes with these disclosures of the causes of disease; with these demonstrations of what I may call the mechanism of maladies, is it any wonder that the attitude of the profession to disease is vastly changed?

The mystery that awed and paralysed us in so many cases 30 years ago has yielded its secret to patient study, and, confident in the new knowledge, we hail,

> The teeming future
> Glorious with visions of a full success.

It is now time to ask, How does this changed attitude affect the problem of medicine—that twofold problem, the preservation of health and the cure of disease? On the latter branch my distinguished colleague, Dr. Broadbent, will address you on Friday. To-day I want to direct your attention to the preservation of health by that prophylaxis which is the primary outcome of all those researches of which I have spoken. These revelations of the causes of disordered health make it increasingly possible for collective effort to be made successfully for prevention, where individual action is comparatively powerless Influenced by this view, the State has legislated, as we shall see, more and more willingly as modern discovery has been able to suggest the methods, and define the objects of preventive measures.

As long as medicine was a slave to the tyranny of theological ideas, or to the mastery of metaphysical explanations, little progress could be made. The shafts of the far-darting Apollo formed as hopeless an explanation of an epidemic as the possession by evil spirits taught down to mediæval times. From such notions nothing better could come than the pathetic isolation of the leper or the organised system of separation, called quarantine. The form of our knowledge was unscientific, and not fitted to lead to systematic prophylaxis. *There was nothing definite to aim at.* The individual practitioner was left alone to contend, as best he could, with the common foe, because science had as yet made no rules for collective action. All this is now altered by the advances made in the identification of the contagia of many maladies, and by intimate acquaintance with their modes of communication and with means suited to check their spread.

When the modern terror, in the shape of cholera, first came in 1831, legislative action could only begin tentatively. As Mead had suggested more than a century before, a Central Board of Health was appointed to organise common measures for the public safety. Necessity has been well called the mother of invention, and in the same sense we may well call panic the parent of sanitation. Each invasion or threatened invasion of cholera has generated fresh legislative activity. At such times the public mind, stirred by fear, has greedily called for the help of the profession, and

consequently there has been produced, with every one of the five cholera alarms, legislation to protect the public health. It was not, however, till the present reign that any systematic recognition of State medicine was made.

On the accession of Her Gracious Majesty, the statute book was practically innocent of sanitary law, except the Quarantine Act. That many Acts have since been added, future historians will regard as one of the greatest glories of the Victorian era. The evolution of this legislation, slow at first, has proceeded more and more rapidly as medical science has created for it an intelligible basis. The movement began in 1842, when the late Sir Edwin Chadwick, who was the father of modern sanitary reforms, startled the world by a memorable report on "The Sanitary Condition of the Labouring Population of Great Britain." The Royal Commission which was appointed in consequence we may regard as the starting point of State interest in public health. Once begun, the progressive movement, though often checked, has never been lost. The public responsibility for the unhealthy conditions in which the population lived was acknowledged once for all, and in 1847 Liverpool, to its honour, appointed the first medical officer of health in the United Kingdom. In the next year, London followed with a similar appointment, destined to be of national importance, for it gave to the service of his country the great veteran of sanitary work, Sir John Simon.[3] Another epidemic of cholera now occurred, and a General Board of Health was established by Parliament for five years. It was in this epidemic of 1848-49 that Dr. Snow began those inquiries which led to the discovery that the intestinal discharges of cholera patients were the means of spreading the disease. The contamination of the water supply by this means was, in the epidemic of 1853 and 1854, again shown to be the explanation of what had been till then the mystery of the distribution of the pest. This famous discovery marks the first stage in the evolution of modern sanitation. The darkness that had so long concealed the modes of conveyance of certain infectious diseases commenced to clear, and in sewage-tainted water was recognised the actual physical evil that accounted for the spread and persistence of many of the so-called filth diseases. There was now something definite to aim

[3] In his recently-published work, *English Sanitary Institutions*, Sir John Simon has given the world a most exhaustive and eloquent history of modern sanitary progress. I gladly acknowledge the valuable help it has been to me in writing this address.

at. Purity of water supply and efficiency of drainage became objects for which sanitarians and legislators could work together.

The General Board of Health appointed in 1848 was renewed in 1854 in a different form, and was continued year by year by renewal Acts till 1858. It is a melancholy reflection that, in spite of the important additions made at this time to our knowledge of the circumstances favouring the spread of epidemics, the Legislature should have still been so blind to responsibility as to regard a department of public health as a temporary expedient. Even in 1858, when the duties of the Board of Health were transferred to the Privy Council by the Public Health Act of the year, Parliament was either so indifferent to, or so ignorant of, the importance of the Act, that it was passed for one year only. Happily the absurdity was recognised in 1859, and a perpetuating measure enacted.

The Legislature had recognised at last, that the care of the public health was a part of its continuous duty, not a task to be taken up in a season of panic, and afterwards laid aside till trouble knocked again at our doors. This was due in great measure to the influence of Simon, who had been acting for some years previously as medical officer to the General Board of Health, and by his great administrative ability and organising power had created a scientific basis for sanitary work.

The late Dr. Headlam Greenhow, one of those who worked with him, began at this time a most important statistical inquiry into the different proportions of death caused by certain diseases in different districts in England. The Registrar-General's returns had hitherto not told more than the general death-rates, and consequently it was impossible to study with the necessary precision the particular prevalence of particular diseases in particular districts. In his report to the Board of Health for the year 1858, Simon admirably enforced the lesson of this inquiry by showing, that the prevalence of many maladies specially affecting certain localities or particular classes of the population was due to the existence of controllable conditions, which special medical investigation by the central authority could discover and correct. This was an unanswerable argument for creating a permanent central health authority to exercise continuous supervision for sanitary ends. The recognition by the State of its duty of maintaining permanently a department of health thus coincides with the next stage of sanitary evolution. There was now added,

to the general duty of maintaining purity of water supply and effective drainage, sure statistical evidence of the greater prevalence of disease in the localities in which these essentials were most defective.

During the next few years the health department of the Privy Council carefully studied, by a system of special inspection, the local distributions of disease, the food supply, the housing, the local physical conditions, and the industrial circumstances of the people, for the purpose of building up a knowledge of the primary essentials of public health. In this work many distinguished members of this Association individually took part, and the public interest in sanitary progress was thoroughly kept alive by the striking lessons drawn from vital statistics by which the late Dr. Farr illustrated his invaluable reports. This Association honoured itself, when it bestowed on him its highest honours in recognition of these labours for the public good.

In 1865-66 another cholera alarm intensified the public concern in health matters, and the Sanitary Act of 1866 made it the *duty* of local authorities to provide for the healthiness of their districts. Thus at last was won the long-delayed recognition of the great principle that the care of the public health is a chief duty of local government. There were at this time in this Association, a number of men, who had for many years paid great attention to State medicine, and who had, some of them, from the beginning maintained the principle laid down by Sir Charles Hastings in 1832,[4] that one of the chief objects of the Association was the study of public health. Two of the foremost and most honoured of these, now no longer with us, were Dr. A. P. Stewart and Dr. Rumsey. Mainly in virtue of their action a Committee on State Medicine was appointed at Dublin in 1867, to act conjointly with a similar committee of the Social Science Association. If the prudent counsels of this joint committee had been followed, much of the legislation of later years would have been anticipated and many of the errors of 1871 and 1872 avoided. One of the first results to follow was the nomination of the Royal Commission of 1868. The report of that Commission recommended the universal

---

[4] The original prospectus of this Association, in stating its principal objects, laid this down in clear terms as follows: "Investigation of the modifications of endemic and epidemic diseases, in different situations and at various periods, so as to trace, so far as the present imperfect state of the art will permit, their connection with peculiarities of soil and climate, or with the localities, habits, and occupations of the people."

appointment of medical officers of health, and led to the concentration in one department—the Local Government Board—of nearly all the relations of the central government with the health of the people.

The great Public Health Act of 1872 embodied some of the recommendations of the Royal Commissioners, and provided for the universal appointment of medical officers of health, and the complete sanitary organisation of the kingdom. This Act created our modern position, and although it has proved disappointing in its working, it remains a monument of the wise constructive statesmanship of its author, Mr. Stansfeld. A return made the next year showed that out of 1,468 sanitary districts, over 1,100 had appointed medical officers of health. As regards the recognition of the office, and the establishment of a universal connection between the duties of the State as regards public health and the medical profession, this was a gratifying contrast with the single medical officer of health in the United Kingdom in 1847. On paper it seemed a result almost good enough for a quarter of a century's work. It was, however, not so satisfactory on analysis of the return, to find that in spite of the encouragement given by the Local Government Board to the appointment of officers for large joint areas, only fifty-eight of the appointments required complete devotion to sanitary duties, and that in hundreds of cases there was only a nominal compliance with the Act. Although the Government estimates for the year 1873 and 1874 voted £100,000 to the part payment of sanitary officials, hundreds of local authorities declined the aid, and selected independence and inefficiency in preference to greater central control. The Act contained, however, as we can now see, the causes of its failure. In the first place it had attempted too much. The sudden and universal appointment of medical officers of health was more than the public intelligence was ready for, particularly in the smaller rural districts by which the appointments were allowed to be made. The Act, moreover, did not contain the powers by which the central authority could remedy defective local action, and advise with commanding authority the petty boards that preferred independence to efficiency; and lastly it made the great mistake of making the sanitary work of local authorities a *secondary*, instead of a *primary* duty. In spite of these defects, the local authorities might have been taught that the highest function of local government is the care of the public health, if the central authority had exercised systematic and frequent supervision of local work.

It is mainly because this has not been done, that we have at the present time both rural and urban districts in this country, that in a sanitary sense are a disgrace to civilisation. In 1883, when a cholera alarm once more excited official activity, the Local Government Board started a systematic survey of the country. It was carried on for two or three years and then dropped, when it had discovered, in the great inequalities of sanitary efficiency, the strongest arguments for its continuance. Some local authorities were described as earnest, others as indifferent and careless, and the apathy was greatest, as a rule, where poverty most afflicted the people and rendered them least capable of self-help. But, perhaps, the strongest argument for such a system of continuous inspection is to be found in the help it gives to zealous health officials, and to earnest sanitary authorities. Such inspection wisely used becomes a great promoter of efficiency by encouraging localities to avail themselves of every new scientific advance. That local authorities daily become more willing to do this cannot be doubted, when one looks over the country and observes, how, in the last few years, sanitary authorities have freely spent money in meeting the modern demand for the isolation of infectious cases. The system of isolation in special hospitals is the last great step in the evolution of public prophylaxis, and is a striking testimony to the influence of modern discoveries as to the nature of contagion.

Parliament has also shown by the Acts which it has passed during the last three years that it is more than ever alive to sanitary progress. The improvements in the qualifications and position of medical officers of health, and the provisions embodied in the Local Government Acts of England and Scotland by which county councils in the future, advised by skilled medical experts, will superintend and protect the health of large areas, are satisfactory evidence of this, and none the less satisfactory because they realise recommendations made years ago by committees of this Association. The measure passed last year for the Notification of Infectious diseases is also a most important step towards that national registration of sickness which our associates, Dr. Rumsey and Dr. Ransome, so long and so ably advocated.[5]

---

[5] The wide voluntary adoption of this Act is most noteworthy. It came into force on August 30th, 1889, and early in July this year it had been adopted in 836 sanitary districts outside the metropolis (where notification was in force without adoption), and had been applied to about three-fourths of the population of England and Wales.

We can freely acknowledge our debt to the President and the Parliamentary Secretary of the Local Government Board, for these valuable enactments, and also for the help they gave to the Infectious Diseases Prevention Bill of this year, which places dairies under medical inspection and will help to check the spread of many diseases conveyed by milk. It is a portent of great significance that measures, which a few years ago would have raised a wide and wild outcry in defence of the liberty of the subject, should now pass almost unopposed. It shows how strongly modern discoveries, as to the nature and mode of propagation of infective disorders, have captivated the public imagination. While the profession could only speak in general terms of filth diseases, explain their distribution by noxious emanations and miasmata, and rely on purity of water and drainage as the only preventive measures, progress was necessarily slow. Since our knowledge of the intimate nature of contagia has developed, isolation has been accepted as a public duty and provided for at the public expense. The stamping out of scarlatina and small-pox, for example, has become a charge upon the rates. The discovery that preventable or infectious diseases are communicated by solid particles of living matter, by specific micro-organisms which never arise spontaneously but always from pre-existing organisms, each forming the specific virus of a particular malady—has given a directness and precision to preventive medicine which has won it popular favour. The relation of these pathogenic microbes to food, earth, air, and water offers the most fruitful inquiry for the public health that medicine has ever begun. The popular intelligence watches the work with the greatest interest and sympathy, because it has understood the meaning of modern methods of inquiry and prevention, and realised that success depends on co-operation between the State and the profession.

It is on this willing co-operation of public representative bodies throughout the country, that we base our hopes of sanitary progress. The day, I hope, is not far distant when that department of the Local Government Board, for auxiliary scientific investigations—instituted with such wise foresight by Simon, and carried on so ably by his successor, Dr. Buchanan—will be voted thousands instead of hundreds for its important work. Years ago it enriched the literature of preventive medicine by the labours of Sanderson, Creighton, Thudichum, and Klein. Of late it has well maintained its character by the original work of brilliant experimentalists, one of the ablest of whom, Dr. Wooldridge, we lately

lost. It is not too much to hope, that one day the nation, out of the superfluity of its wealth, will adequately endow a department for researches, which bear so directly on the health and wellbeing of the richest as well as the poorest of her citizens. It is by such a department—adequately endowed—working in connection with a staff of medical inspectors, supervising the work of medical officers of health, able to devote their whole time to their duties, that the administration of public health will be finally perfected. We are still unhappily far from this condition. I find that this year there are in England and Wales 1,545 medical officers of health, receiving annually nearly £100,000, but of these only 101, or less than 1 in 15, are health experts, in the sense of being free from practice and solely engaged in sanitary work. There is, therefore, much to be accomplished. It cannot be long deferred, for the duties of a health officer must daily grow more specialised, and when he becomes a recognised specialist, and not a competitor in practice, his work will receive more and more assistance from the profession. His aims and methods can never be a matter of indifference to the great body of the profession, for he should be the useful ally of the practitioner, on whose knowledge and discernment in individual cases he must always depend. The practitioner will necessarily be the first to discover and recognise preventable disease, and the function of the health officer will be to support and complete his action.

Thus the relations between the State, the health officer, and the practitioner will grow closer, as our organisation is completed in that single-minded co-operation for the public safety, which science and humanity alike demand. The relations, as they develop, will need for some time to come, however, the most watchful care. It is fortunate that this Association has, in its Parliamentary Bills Committee and its able chairman, a representative body, admirably fitted to safeguard medical interests and to speak with unequalled authority for the profession. The British Medical Association has borne so important a part in fashioning legislation in the past, that I doubt not, it will have an increasingly honourable share in perfecting such work in the future.

It seemed to me that if it was excusable anywhere to speak in this address on these topics, Birmingham was the place. Here the Social Science Association began its life under the presidency of Lord Brougham, and amid the congenial surroundings of a population eager for social reforms. Here, too, that Association closed a distinguished career, after having taken a noble part in winning the legislative advances to which I have referred.

In Birmingham, moreover, we have a striking example of a community which has always given special consideration to social questions, and by virtue of this quality has formed a high ideal of local government. The title of the "best governed city," given to the town by a recent American visitor, shows that high aims have led to high success. When the Association last visited this town in 1872, Mr. Joseph Chamberlain and his colleagues were just beginning the memorable enterprise of remodelling the municipal government, which has resulted so happily in the transformed aspect of the city. The transformation is not limited to the surface: it has penetrated deeply into the lives and homes of the people, and has given the world an example of how a great self-governing community, by the loyal co-operation of its citizens from the poorest to the richest, can increase intelligence, cultivate taste, elevate morality, and purify the physical conditions of life. In the free libraries, in the schools—elementary, art, and technical—in the parks, and in the improvement area which once constituted the slums, there is evidence of the results of the modern spirit of municipal work. In no department has this work been done more efficiently than by the Health Committee. The results are shown in a death-rate which has fallen from 26.6 per 1,000—the average of the twenty years ending 1870—to 23.5 for the ten years ending 1880, and 19.6 for the nine years ending 1889. Thus, while the population has grown in numbers during the last twenty years by about one-third, the annual general death-rate has lessened by 7 per 1,000, or by more than one-fourth. The zymotic deaths have fallen in the same time from over 7 per 1,000 to less than 3. In those parts which now form the improvement area, and which once were slums, the death-rate has fallen from over 50 to less than half, or 23.1 per 1,000. These figures tell more

eloquently than words, how the wise expenditure of public money, comes back after not many days in the lessened sorrow and suffering of the population.

The question, Am I my brother's keeper? has been answered in Birmingham in a spirit of true fraternity. The sympathy for misery, which has shaped so much of the national legislation in recent years, has found in this city one of its noblest monuments. Like other towns, we have hospitals and charities of which we are proud, but we have also one of the most perfect Poor-law infirmaries which this kingdom has yet seen. Everyone will, I hope, visit the noble hospital, which has been erected at a cost of over £100,000 as a refuge for sick paupers. A workhouse hospital does not usually connote beauty and comfort, but in the Birmingham Workhouse Infirmary of to-day, both are present. Splendid and spacious corridors connect thirteen blocks of wards, each block the size of a recognised clinical hospital. In the wards you will find the patients bedded amid bright and cheerful surroundings, that would do credit to any of the great institutions of the metropolis. Everything that science and art can contribute for their relief is supplied, well-trained, cultivated ladies serve as their nurses, and two of our most able colleagues act as visiting physician and surgeon.

Go and see, I ask you, how Birmingham takes care of its 1,500 sick poor, and you will, I am sure, appreciate the wise benevolence that has raised this great medical monument of Christian charity. There is, to my mind, only one defect: every one who is admitted is forced to become a pauper. I hope one day sickness and suffering will be sufficient passport, and that the benevolence which proffers the relief, will not mar the sweetness of the gift by a condition which embitters its receipt. When that day comes, and the collective provision for the sick is made a public duty, many hospital abuses will be cured by the municipalisation of our charities. The isolation of infectious cases has already been put upon the rates without entailing pauperism, and our City Hospital contains some 400 beds. Thus these two rate-aided hospitals receive some 2,000 patients, or between three and four times as many as are maintained in voluntary hospitals. In London a similar condition exists, for there nearly two-thirds of the 18,000 in-patients are supported from public funds!

Some of you may say I began this address with philosophy and I end it at a workhouse—a goal which philosophy some-

times attains. I accept the criticism. It states a truth and conveys a lesson. It is that lesson which I wish to impress upon my professional brethren—the immeasurable importance of even their highest scientific work to the well-being of the poor, and through that to the stability and prosperity of the State. We have in our ranks some 4,500 Poor-law medical officers, who are the daily bearers of succour to suffering thousands, and in many cases the only visible link between the rich and powerful State and the homeless and hopeless poor. In the daily work o_ these 4,500 doctors there reside greater potentialities for social progress than in any other class. To them is given the highest function committed to us by the Highest—the care of the suffering poor—and in the daily discharge of that holy task, the vast hecatomb of human misery that civilisation piles up may be diminished and prevented. If it is not so prevented, decay will come upon us as a nation; for never yet have strength and stability been found in hoarded wealth, but only in the content and comfort of the poorest classes of the population. I want every parish surgeon, every practitioner among the poor—and thank God we all have poor patients—and every medical officer of health, to realise the nobility of the service he gives the State; and one day, I hope no distant day, the State will awaken to the value of such service, and recognise in the trusty dispenser of a nation's charity, or the wise saviour of a city's health, servants of the State more worthy of its honours than the successful soldier, or the astute diplomatist.

But come this higher hierarchy of worldly honour soon or late, to men who do their duty in our ranks, there will come one day the grandest words of welcome and reward, "Well done, thou good and faithful servant; enter thou into the joy of thy Lord."

London: Printed at the Office of the British Medical Association, 429, Strand.

BY THE SAME AUTHOR.

8vo, ILLUSTRATED.

CLINICAL MEDICINE:

LECTURES AND ESSAYS.

CONTENTS: ULCER OF THE STOMACH — CYANOSIS — ETHER IN PHTHISIS — DIGITALIS IN HEART DISEASE — RUPTURE OF HEART VALVES — ACUTE RHEUMATISM — DIABETES — PSEUDO-HYPERTROPHIC PARALYSIS — THE SPHYGMOGRAPH AND CARDIOGRAPH — THORACENTESIS BY ASPIRATION.

8vo.

METHOD AND MEDICINE:

AN ESSAY ON THE PAST, PRESENT, AND FUTURE, OF MEDICINE.

8vo.

THE POLITICAL POWERLESSNESS OF THE MEDICAL PROFESSION:

ITS CAUSES AND ITS REMEDIES.

J. & A. CHURCHILL, NEW BURLINGTON STREET.

MESMERISM:

(HYPNOTISM)

Its Possibilities, its Uses and Abuses.

BY

SIDNEY BARWISE,

M.B., M.R.C.S., L.S.A.,

PRICE SIXPENCE.

BIRMINGHAM:
CORNISH BROS., NEW STREET.
HUDSON AND SON, EDMUND STREET.

1888.

THE subject matter of this pamphlet is taken from a paper read before a meeting of the Queen's College Medical Society, and it is printed at the unanimous request of that meeting.

The subject is of such public interest that I have re-written the paper in popular language. I hope before long to be able to publish, at greater length, the results of some investigations upon this subject.

S. B.

MESMERISM.

IN all ages and amongst all sects—Buddhist, Brahmin, Mahommedan and Salvationist alike—certain enthusiasts have been in the habit of inducing in themselves a peculiar ecstasy, or trance-like state. An identically similar condition can be induced by the means adopted by the professional mesmerist; which state is then known as hypnosis or induced catalepsy.

The scientific study of the phenomena of this condition has long been neglected. We are now, however, thanks to the investigations of Braid, Carpenter, Maudsley, Hack Tuke, Charcot, and others, in a position to give a scientific and systematic account of the symptoms and causation of this state.

Under such names as mesmerism, animal magnetism, electro-biology, and odic force, many wonderful cures, which have been looked upon as well nigh miraculous, have been effected. So, too, by the laying-on of hands, the discordant jargon of sorcerers, the horrible concoctions of the Rosicrucians, and the little pilules of the so-called homœopathists, all manner of diseases have been cured. These, however, are only instances of the power of the imagination in modifying the action of the trophic centres* of the brain.

When alchemy had had its day, animal magnetism took its place, and found many apostles amongst those who had formerly been seekers of the philosopher's stone and the elixir of life.

Paracelsus boasted of being able to remove diseases from the bodies of men into the earth by means of magnets. In

* Certain centres in the brain presiding over the nutrition of the tissues of the body.

1771, Father Hell, a Jesuit and professor of astronomy at the University of Vienna, constructed steel plates* of a peculiar form, which he applied to the body as a cure for various diseases, and three years later he initiated Anthony Mesmer into his secrets. Mesmer modified Hell's theories, established a system of his own, and published, in 1779, an account of his theories in twenty-seven propositions regarding the nature and curative effects of animal magnetic fluid.

"It is impossible," says M. Dupotet, "to imagine the excitement Mesmer's experiments created in Paris. No theological discussion was ever conducted with such bitterness."

Attacked on the one hand by the entire faculty of Medicine with the exception of M. Deslon (the first physician of the Comte d'Artois), he was looked upon as a quack, a fool, and a madman; the Abbé Fiard affirmed that he had sold himself to the devil. Supported, on the other hand, by many wealthy patrons who were as extravagant in his praise as his enemies were bitter in their abuse, the battle was keen while it lasted.

Mesmer lived just at a time when the marvellous was readily believed in; balloons and lightning conductors had just been invented, and no one knew where science ended and imposition began. From amongst the dissolute pleasure-seeking and debauched loungers, and the dilettanti of dissipated Paris, he attracted to himself a wealthy and an influential, though an idle, clientele. His method was admirably calculated to entice those fashionable hypochondriacs who are to be found in all large cities; and there is no doubt that many went to him who had drained the cup of pleasure to its dregs, in the hope of exciting some fresh sensation, and his system was equally adapted to satisfy this desire. His house† was a magnificently furnished mansion, its walls were covered with costly tapestries, and curtains of the richest silks and velvets; the windows were

* At the present day, every imaginable ailment is cured (according to advertisements) by electric belts, which, as a matter of fact, generate no electricity.

† See Histoire du Merveilleux, vol. II.

of exquisitely tinted glass, which shed a soft and mystic light into the spacious salons; there it was reflected in a hundred directions by mirrors hung in suitable positions, and lighting up the fantastic hangings, produced an almost magical effect.

The air was heavy with the scent of orange flowers, mingled with fragrant incense burning in antique vases, swinging from the ceiling.

A constant ripple was kept up by a fountain playing in an adjoining apartment, the softest music fluttered from æolian harps, or a girl's sweet voice swelled forth at certain periods. The impression produced was enchanting.

The patients, who were seated round the celebrated *baquet*, a kind of bath in the centre of the room, after a short space of time became almost intoxicated with these voluptuous and sensuous effects. Then, the assistant magnetizers, strong, handsome, well-proportioned men, came in. They were supposed to transmit the magnetic fluid to their patients by sitting opposite to them, staring them full in the eyes, rubbing them gently down the spine, and in the course of the nerves. Gradually the cheeks of the subjects flushed, their eyes brightened, their pupils dilated, their bosoms heaved, their eyelids closed, and they either became unconscious or went off into convulsions. At this crisis Mesmer came upon the scene, dressed in a magnificent gown of lilac silk, carrying in his hand a rod of iron, with which he affirmed he magnetized the seats of disease in the bodies of the palpitating crowd around him.

The government appointed a commission to enquire into mesmerism; and in a report, which was drawn up by the illustrious astronomer Baily, and was a marvel for its strict and impartial reasoning, the conclusions arrived at by the commissioners were as follows:—That no real cures had been effected, that the diseases supposed to have been cured were imaginary maladies, that there was no magnetic fluid at all, but that all the phenomena were due to the imagination and expectant attention, and that the excitement seen in the women was due to that power which nature has implanted in the one sex to arouse deep emotion in the other.

This report was the death blow to mesmerism and in consequence of it, men held aloof from this subject and hardly any real progress was made until Braid, of Manchester, published his masterly works. Braid conclusively showed that when a patient was hypnotized it was not by the influence of any subtle fluid which emanated from the nervous system of the operator. He had the most remarkable success in the treatment of disease, perhaps his most wonderful achievement being that of giving hearing to a deaf mute who had for years been an inmate of a deaf and dumb institution. Since hypnotism has been employed in the treatment of nervous diseases, and has recently been thoroughly investigated by Charcot at the Salpêtrière with some remarkable results which, if proved to be true, will cause the phenomena once more to be relegated to the domain of animal magnetism.

One does not have recourse at the present time to the elaborate methods and voluptuous measures of Anthony Mesmer to induce hypnosis. There are several conditions necessary for its induction; in the first place, the subject must have a peculiar *susceptibility*. Little is known about the characteristics which mark the susceptible; I believe they can be recognised by having bright 'speaking' eyes, large pupils, a vivacious manner, a tendency to venous congestions, and the habit of blushing easily: in short, they are what is generally called *nervous*. They are versatile, quick and intelligent; but emotional, erratic and extravagant. Women are more easily hypnotized than men, the young than the old. They will be found amongst those religious bodies, which, like the Salvation Army, give way to emotional excitement. Certain races are more susceptible than others, for instance, French women are more easily hypnotised than English, and will pass into a more profound hypnosis.

The muscularity of the subject does not in any way affect his susceptibility. Mr. Hansen, a most successful mesmerist, states, that English students who row, swim, and ride, are more easily hypnotised than their German compeers who lead a more sedentary life, in fact no difficulty has been experienced in hypnotizing several professional pugilists.

It is commonly supposed that only persons of weak intellect or little resolution can be hypnotised. This idea is entirely erroneous; as long as the subject is willing and has the power of concentrating his attention, successful results can be obtained with the most intellectual.

On the contrary, Dr. Jänicke states that lunatics are not at all susceptible. I have tried several times to hypnotise imbeciles, and have only been partially successful; hysterical and hystero-epileptic patients are, *par excellence*, the best subjects. The influence of drugs in aiding or preventing hypnosis has not, as yet, received a proper amount of attention. I have had, morever, no difficulty in hypnotising patients taking bromide of potassium, or choral hydrate, and from some observations I made a year or two ago upon the action of cannabis indica in the treatment of sick headache, I was led to the conclusion that this drug predisposed to hypnosis, and a recent experience has removed all doubts I might previously have had. Heidenhein was able to hypnotise his brother and Dr. Kröner, whilst they were inhaling nitrite of amyl, a drug which causes congestion of the brain, an identically opposite result to that of bromide of potassium.

We must content ourselves with this brief statement of the predisposing causes of hypnosis, and now pass on to the consideration of those exciting causes which are immediately instrumental in the induction of this state.

First of all, it is necessary most emphatically to state that there is no animal magnetic fluid, nerve force, odic force, or any subtle fluid of any kind, which passes from the operator to the subject.

In fact, the phenomena may be induced by the subject simply staring at a bright object attached to a coronet around his head in such a way as to cause an upward convergent squint, without the intervention of any operator. The ticking of a watch, the passing of the hand regularly in front of the eyes, in fact any sensory impression kept up for some time, or even a short powerful stimulus, such as the sound of a loud gong, or the bright light caused by the incandescence of magnesium ribbon, is sufficient with those predisposed to cause profound hypnosis.

What happens when the eyes are fixed up on the bright object?

On account of its brightness tears flow, the vision becomes dazed, objects at the borders of the field of vision first disappear, and finally the object itself. The subject, who previously expects to become unconscious, finds himself seeing nothing, although he knows his eyes are open. He accounts for this by believing himself asleep. His pupils dilate, there is a little trembling of the eyeballs, they rotate upwards, the eyelids first twitch and then fall.

These changes are habitually associated with sleep, and it is easy to conceive how other changes, which generally accompany them, follow in their wake, and culminate in an artificial sleep (hypnosis).

We cannot positively know anything of the exact mechanism of the production of this state, we can, however, form some idea of it from the study of those conditions in man and the lower animals which most resemble it. For more than two hundred years it has been known that if a cock or hen be grasped firmly with its beak to the ground and then a straight line be drawn with a piece of chalk from the tip of its beak so that its eyes converge upon it, it will remain so insensible to pain as not to feel the prick of a needle. Czermak has succeeded in mesmerising swans, turkeys, and ducks, by means of fixing their attention upon some bright object. If a frog be placed upon its back and a piece of cotton tied round his legs, it will remain perfectly still till it dies, without making any effort to save itself. The same thing happens in nature when the cat fascinates a bird, or the boa-constrictor its prey, which makes no effort to escape, and even advances a willing victim to its enemy. These instances throw considerable light upon the way in which the hypnotic state is induced.

Maudsley states that it might be set down as a general law "that given two nerve centres of mental function, they cannot both be in equally conscious function at the same time; if the one is actively conscious, the other will be subconscious, or not conscious at all—it will be rendered temporarily incapable of function."*

* The Pathology of the Mind—chap II, p. 58.

Thus an acute pain renders us insensible to a lesser pain. So by fixing the consciousness by some act of concentrated thought, or stimulating one receptive centre alone and removing all other sensorial stimulations, there is a subsidence in the activity of all the brain except the receiving centre of the stimulated nerve; and when this one nerve centre is so tired out that it can no longer perform its functions, total insensibility results. But this explanation fails to account for those instances of hypnosis being induced by a sudden flash of light, or a commanding gesture, as employed by the Abbé Faria, in which cases no other theory can be devised but that of a sudden alteration in the blood vessels of the brain, either in the direction of Anæmia, * the brain being blanched as in sleep; or congestion,—a blushing of the brain,—as in some cases of epilepsy. Or, possibly, it it may be due to a sudden dilatation of the veins of the membranes of the brain, causing temporary cerebral compression. That there is a remarkable relation to epilepsy is shown by the following facts: in both, there is a convergent squint followed by dilated pupils; in both, the face and neck are engorged with blood, and the hypnotic state may be substituted for epileptic seizures. There is on the other hand a still more striking relation to ordinary sleep. If a patient is hypnotised and left alone, he will pass into an ordinary sleep; on the other hand, ordinary sleep may in some persons be converted into the hypnotic state by suitable means. Again the methods of inducing both are identical. The women of Brittany send their babes to sleep by hanging glass beads from the cradles for them to stare at. The regular oscillations of the cradle, the monotony of the lullaby, and the slow drawl of some preachers produce their soporific effects in the same way.

The most marked difference between ordinary sleep and the hypnotic state is, that in hypnosis the subject will receive suggestions; for instance, if told he has neuralgia, he will believe it. In ordinary sleep, however, we are open

* Anæmia (bloodlessness). The condition of the blood vessels of the brain is ascertained by examining the expansion of the brain—as optic nerve—in the eyeball. In sleep and during hypnosis the arteries are contracted and the veins dilated.

to suggestions to a certain extent. Alfred Maurey believed he was in Jean Farina's shop at Cairo when Eau de Cologne was put to his nostrils during sleep. Dugald Stewart applied a hot water bottle to the feet of a friend, who dreamt he was climbing Mount Etna. It appears that he burnt his feet whilst ascending this mountain some years before. And when experimenting upon myself with cannabis indica, a drug which predisposes to hypnosis, I imagined the noise caused by someone turning over the leaves of a book, to be a loud thunderstorm.

Until we know more of the physiology of sleep, we shall be unable to get any nearer to the intimate mechanism of the exact changes which produce hypnosis.

There is one factor which is too frequently overlooked in the consideration of various nervous conditions, and that is, that the skull cap, unlike the capsule of every other organ, cannot expand with variations in the size of the organ it contains and the pressure which is exerted upon it from within.

Now if the veins of the membranes* of the brain become engorged with blood, as the skull cap cannot expand, they must exert direct pressure upon the brain itself, and compression of the brain results in insensibility, whether it be due to the pressure of a fractured skull or a hæmorrhage upon the surface of the brain, a collection of pus, or any other cause of cerebral compression. Some such engorgement of the membranes of the brain, I believe, takes place during hypnosis, and this view is supported by the following facts:—The susceptible have a tendency to venous congestion; they are subject to sudden changes in their blood-vessels, thus they readily blush; during hypnosis there is suffusion of the features, and persons are most easily hypnotized when their heads are bent well back so as to obstruct the return of blood from the brain. In fact, all that is necessary with some susceptible persons is to suddenly bend the head back and they will pass into a deep sleep. We shall revert to this matter again when we come to consider the cerebral circulation.

---

* The pia mater is referred to. Of course there is the sub arachnoid fluid, but this is practically incompressible. See Dr. Cappie on Causation of Sleep.

However the state is induced, it assumes one of three types which Charcot has named:—Lethargy, Catalepsy, and Somnambulism.

1. The Lethargic State.

The subject becomes flaccid as if plunged in deep sleep, there is insensibility to pain, the eyelids are closed and the pupils contracted, and it is impossible to affect the subject by suggestions.

There is exaggeration of the tendon reflexes* and what is termed by Charcot neuro-muscular hyper-excitability.†

2. The Cataleptic State.

This may be induced primarily, or consecutively to the lethargic state, by raising the eyelids.

The subject is motionless as if fascinated, the gaze is fixed, tears flow down the cheeks, the conjunctival reflex is absent, the pupil contracts to light, the limbs remain in the position in which they are placed, for a longer time than it would be possible to retain them in were the subject simulating catalepsy.

The tendon reflexes disappear, and neuro-muscular hyper-excitability is absent; there is continuance of sensorial activity. The subject may be caused to perform automatic actions by means of suggestions.

3. The State of Artificial Somnambulism.

This is generally induced secondarily to lethargy by friction upon the scalp, or, in rare cases, immediately, by fixity of gaze or passes after the style of professional mesmerists.

It is characterized by insensibility to pain, absence of neuro-muscular hyper-excitability, and extreme acuteness of sight, hearing, smell and muscular sense. The subject can be caused to perform any act at the suggestion of the operator, and hallucinations can be induced by suggesting them. It may be re-converted into lethargy by slight pressure upon the eyelids.

These three states are not distinctly separated, but pass

* The involuntary jerk of the foot, produced by striking the skin on the front of the knee, and known as the knee-jerk, is a tendon reflex.

† This term is fully explained later on. It is an increased irritability or tendency to contract on the part of the muscles, when they or their nerves are subject to mechanical irritation.

gradually into each other, so that it is often difficult to state where one stage ends and another begins.

We will now examine in detail the symptoms and physical signs of hypnosis, noticing more especially the objective phenomena of this state as they form a perfect safeguard against imposition.

The Circulation.

Victor Horsley hypnotized Mr. North, the lecturer on Physiology at the Westminster Hospital, and on taking sphygmographic tracings of the pulse, found there were no alterations.

The pulse is increased in frequency, and Hack Tuke states that if the arms are kept extended above the head for five minutes, its rate is doubled, while it is only slightly increased when the arms are voluntarily held in that position.

The Cerebral Circulation.

Heidenhein first of all believed that the brain was anæmic*; this view was supported by examinations of the retina, which showed the arteries were contracted and veins dilated, and by the fact that persons can be hypnotized when taking bromide of potassium, a drug which causes anæmia of the brain.

But, on the other hand, Heidenhein was able to hypnotize his brother whilst inhaling amyl nitrite, and as this drug dilates the arteries, he was led to the directly opposite conclusion to that which he first formulated. I think these apparently contradictory results may be reconciled by remembering that nitrite of amyl, by lowering the blood tension, leads to congestion of the membranes of the brain, thereby causing anæmia of that organ, as before explained.

Respiration.

In Lethargy the respiratory curve does not materially differ from ordinary quiet breathing.

In Catalepsy, however, the movements are infrequent and of slighter amplitude than normally.

The difference between the two respiratory curves is shown in Fig. 1.

---

* See Page 9.

Fig. 1.

The upper curve represents the respiratory curve in Lethargy, the lower curve that of Catalepsy.

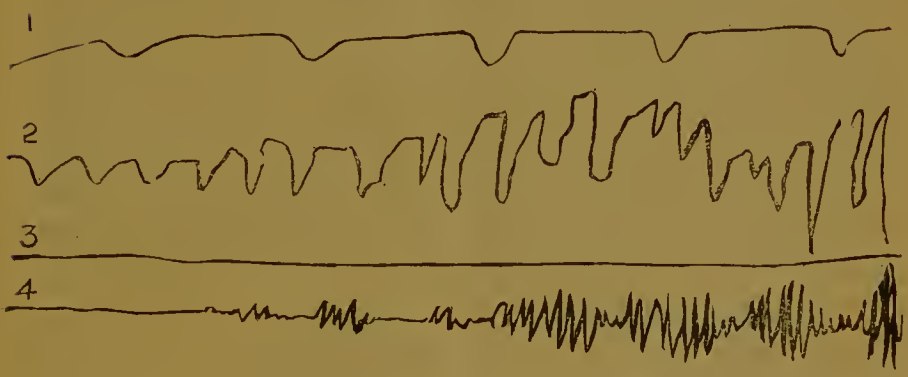


Fig. 2.

1. Respiratory tracing during Catalepsy.
2. Respiratory tracing from a man who attempted to maintain cataleptic attitude.
3. Tracing to show oscillations of arm during Catalepsy.
4. Tracing of oscillations of arm of same man.

[*After Charcot*].

Tamburini and Leppili maintain that the curves in lethargy are modified by applying a magnet to the epigastrium, but until these experiments are repeated upon this side the channel they must be received with caution.

If a suitable apparatus be applied to the extended arm of a patient in catalepsy, and the respiratory bracings be taken at the same time, it is found that the cataleptic arm does not tremble, and the respiratory curve maintains the normal character it did before the experiment; on the other hand, an individual who voluntarily maintains this position

soon shows signs of fatigue, and his breathing becomes rapid and irregular. (*See Fig. II.*)

These objective signs it is impossible to imitate, and they prove a ready method of guarding against imposture.

Neuro-Muscular Hyper-Excitability.

Charcot pointed out that when a subject is in the lethargic state, and the muscles or their nerves are subjected to mechanical excitation, a contraction of those muscles follows, and to this phenomenon he gave the name neuro-muscular hyper-excitability, and he regards it as the classical sign of lethargy.

Thus if the skin over the ulnar nerve is rubbed, flexion of the ring and little fingers takes place ; if the muscles on the front of the forearm, the hand becomes flexed upon the arm ; and stroking the skin over the sterno-mastoid muscle causes the subject to become wry-necked. When this contraction is once set up it continues throughout the lethargy. Heidenhein demonstrated upon his brother that by stroking the ball of his left thumb a spasm spread all over his body in the following order :—

Left thumb	Left leg
Left hand	Left thigh.
Left forearm	Right thigh
Left arm	Movements of chest
Right arm	ceased
Right forearm	The neck
Right hand	

The spasm at once ceased upon striking the arm forcibly.

Heidenhein believes that if the experiment were not stopped, the spasm would spread to the muscles of respiration and might cause death. Such a dangerous experiment as this does not require repeating and could only be verified by some one, who, like the younger Heidenhein, knows the danger and expresses his willingness to submit himself to it.

The muscular power exerted during this contraction is much greater than can be exerted voluntarily. Mr. North, B.A., to whom reference has been made, was, during hypnosis placed with his head upon one chair and his feet upon another. He states that while he was in this position

he heard Mr. Hansen express his determination of sitting upon his legs, and writes "I remember wondering whether the posterior ligaments of my knee joints would give way, knowing the strain to be put upon them was fifteen stone." Increased muscular power too is easily demonstrated by causing the subject to pull at a spring balance or by means of the dynamometer.

The Special Senses.

Sight is generally completely abolished, the pupils are contracted, and the conjunctival reflex absent. One of my subjects when told to write his name, commenced and wrote part of it, when I snatched the paper away, he finished writing his name in the air as if nothing had happened.

That he does not get correct mental pictures is plain, too, from the fact that he will walk into a wall—believe a flower to be a dog, or a dog to be a baby. Charcot gives an objective sign, which, if one could always obtain, would be absolutely convincing as to the genuineness of the phenomena. He draws a cross on a piece of plain paper and tells his subject it is some particular colour, orders him to look fixedly at it for some time, and then closes his eyes, and the subject sees the complementary colour of the one he has imagined he was looking at. Thus if told the cross is red, the cross he would see afterwards appears blue-green, if told it is a green-yellow he would afterwards see a violet cross. Now if these results are obtained with an ignorant person they are absolute proofs against imposition, as it is impossible to conceive that an ordinary factory hand could have an intimate acquaintance with the theory of complementary colours. I have tried to verify this sign but have never been successful.

Hearing is the last special sense to be affected, it becomes so acute, according to Azam, that a watch can be heard ticking 30ft. away. It is quite plain that the subjects hear, for they will act upon the suggestions made to them by the voice.

Smelling.—All the subjects experimented upon in England have more or less complete loss of smell, when tried with ammonia, Eau-de-Cologne, and musk. These results have been obtained by Mr. Lawford, at Bethlem,

Mr. Hack Tuke, and Mr. Price, of Guy's Hospital, and I have verified them myself. M. Taguet states that in a case of his there was such hyperæsthesia that the subject could return a number of articles, coins, keys, gloves, etc., to their rightful owners, by smelling the articles and then their owners.

Taste.—This is entirely lost: a subject will eat mustard, pickles, candles, with no sign of distate. A subject of mine would take quinine, appearing to enjoy it, when told it was sugar, but at once recognized its bitter taste upon being roused.

The Muscular Sense is said by Azam to be much increased. He gives a case of a person threading a needle, with a book placed between it and his eyes.*

Feeling.—Not only complete loss of tactile sense but complete anæsthesia may be induced. As early as 1829 Cloquet amputated a breast under hypnosis, in 1849 Loysel amputated a leg and excised some diseased glands in the neck. Guérineau performed an amputation through the thigh, the patient afterwards declared that although he knew what was happening he felt as if he were in heaven.

Heidenhein hypnotised his brother, who then had a tooth extracted painlessly, and the experiment has been repeated in this town with perfect success.

Having examined the symtoms and signs of hypnosis in something like a systematic manner, we will now examine the most remarkable phenomenon of this state; a phenomenon so incredible, apparently so unintelligible, that at one time scientific men rejected it as mere charlatanry.

When a person is in a state of artificial somnambulism it is possible to make him feel, speak, taste, and act as the operator pleases to suggest.

To understand how this happens we must remember the experiments of Maurey, already cited, on natural sleep.

We know that stimuli reaching the brain during sleep will not be corrected by appeals to the other senses, nor to the accumulated experience derived from previous sensations.

---

* We know that in ordinary sleep the muscular sense is so exaggerated that sleep walkers will traverse dangerous and precipitous paths without faltering, along which they would never dare to tread when awake.

The stimulation caused by a flea bite made Descartes dream he had been run through the body with a rapier, whilst fighting a duel.

The part of the brain which analyses stimulations, checks them by appeals to memory, and compares them with other stimulations, is asleep. And just as in the natural sleep the idea produced by any stimulus, however absurd, is accepted as true; so in hypnosis the idea produced by an impression reaching the brain through the ear is accepted, —without weighing the 'pros' and 'cons,'—as the sternest reality itself.

It is interesting to find that there is a limit to the extent to which suggestions will be received; and just as a loud noise or strong pain will arouse a person from sleep; so a suggestion, which causes too great a shock to that brain tissue which performs the functions of ideation, will be rejected, the subject often waking.

Thus Charcot states that most subjects will not perform any indecent or criminal act. This is most interesting, as shewing that we have in hypnotism a method of gauging the moral control of individuals. Thus a subject of mine, a total abstainer from alcohol, could not be induced to drink from a cup which she was told contained beer. If I had persisted and had succeeded in making her take it, I should have beaten a track for nervous impulses to descend from the brain and rendered the repetition of this act easier at any time. But there is another side to this picture—by suggesting that my subject could not swallow beer, disliked beer, would be sick if she drank beer, I could increase the power which the brain had to inhibit this act.

It is interesting to notice that French subjects will go much further than English subjects under hypnotism. Thus my subjects will smile if told they are several hundred years old, and then asked how old they are; but in Paris they immediately reply as they have been told, a century or two as the case may be. Professor Liégeois has performed some experiments with a view of showing the possibility of crimes having been committed unconsciously by persons who had been hypnotized, and then been commanded to commit them after they came round.

He has made them sign their names to documents, give receipts for large sums of money, administer a white powder they believed to be arsenic, and he records one case of a girl firing a revolver, which she believed to be loaded, at her own mother.

These experiments are dangerous in themselves, they have never been, and I hope never will be, repeated in England. They have been useful, but there is no necessity to repeat them. When I first read them the alarm I felt was great, lest criminals might learn that they had in hypnotism a method of obtaining dupes.

But, upon consideration, it is clear that this very secrecy is its danger. For, in the first place people may at the present time allow themselves to be hypnotized by persons they do not know, without having their friends present; a preceeding I cannot too emphatically condemn; and in the second place, the possibility of such a state of affairs has never been recognised by the English Bar, or police authorities.

Not only can suggestions be made through the ear, but by association. If the hands are placed in the attitude of prayer, the position which is generally associated with this act is assumed; or if the hand is clenched and put in a threatening attitude, the brow will be knit and the canine teeth exposed. The parts of these acts are habitually associated, and impulses descending from the brain to keep one part of the act up escape along the other channels which they are accustomed to.

Again, advantage is taken by the professional mesmerist of that inherent tendency to imitate which is present in all of us, and has free play when the inhibitory action of the brain is lulled by hypnosis. Thus, the mesmerist smiles, the subject's face passes into a broad grin; the mesmerist feigns a pain in the side, the subject rolls in agony upon the floor, and so on. This is the chief stock-in-trade of those mesmerists who pretend to have a seventh sense.

The most interesting part about the study of suggestion is that its effects may last after hypnosis is over; thus, if it is suggested that the subject is paralysed in one arm and will continue paralysed when roused from hypnosis, he will be unable to move the arm when he comes round.

Charcot in this manner produced a paralysis of the right side of the body, roused his patient, and found to his surprise that beside paralysis of the right side of the body his subject was unable to speak. This is usually the case with paralysis of the right side of the body, the reason for it being that the portion of the brain moving the right arm is in close relation to that moving the lips and tongue.

These paralyses are at once stopped by the operator re-hypnotising the subject, assuring him that he is all right again, and will continue all right after he is roused.

The paralysis induced does not in any way differ from an ordinary ideal paralysis, which can therefore be cured (when it has resisted other treatment) by this method. The cure can be made permanent by also adding that the arm will never be paralysed again.

I had, upon two occasions, induced a paralysis of the left arm of one of my subjects. I re-hypnotized him, and then assured him the arm could never be paralysed again. I was never able, afterwards, to succeed in again paralysing the limb.

When examining the phenomena of suggestion, we have seen that it is possible to affect such a change in the brain, under hypnosis, that the subject upon being roused has been compelled to act as the operator has suggested. We have seen that it is possible thus to cause a person to commit a crime—the idea of turning this power to useful account has only lately been realized.

A confirmed drunkard, whose brain tissue has become so degenerated, and will power so enfeebled that he has been on the brink of insanity, has been hypnotized. It has then been suggested to him that he cannot swallow alcohol, that he dislikes alcohol, that he abhors the smell of alcohol, and that it makes him ill.

He is then roused, alcohol is offered to him which he refuses, and we know that each time a nervous action takes place its repetition becomes easier. In time the inhibitory nervous path becomes as it were well trodden, so that a feebler impulse will suffice to check an action than could have done so previously. The periods between the hypnotizations are increased, until it is no longer necessary, and thus the

vicious circle of events is broken, the will is fostered, the brain regains its former vigour, and the health returns.*

Not only is this process applicable to the cure of intemperance, but may be employed upon the same principle for the eradication of vicious tendencies and bad habits.

Thus M. Liébault hypnotized a boy of considerable intelligence, but an incorrigible idler; it was suggested to him that he must work to get to the top of his class. He rose to the top of his form, and maintained this position for several months. The suggestion then began to wear off, and, as the boy had by no means enjoyed the part he had been forced to play, he refused to be hypnotized again.

I am not going to suggest that hypnotism should take the place of the birch-rod, but I feel certain that it may prove of considerable use in the breaking in of unmanageable boys, and are we not justified in hoping that it might be employed in the training of young criminals in reformatories. We know that vicious self-indulgence is one of the most potent factors in the production of insanity; before its wretched slaves are excluded from society, surely as a final resource hypnotism might be tried.

In the treatment of epilepsy we have in hypnotism a method, the possibilities of which are great. My attention to this subject was called by a fellow medical practitioner in this town, who told me of a lady, a confirmed epileptic, whose disease had resisted all treatment, and who had travelled with her mother through England in order to be hypnotised regularly by a professional mesmerist. Her fits became fewer, she was hypnotised less frequently, and eventually they ceased altogether. She is now married, in good health, and the mother of a family of healthy children. I have employed hypnotism in the treatment of four cases of epilepsy, two were congenital cases of epileptic imbecility, neither of these was affected at all; of the other two one is completely cured, the other only has a fit about once in three months as compared with one or two seizures a week previously.

M. D'Anquier, a mesmerist, who was formerly one of Her

* This is no day dream or flight of the imagination, but has actually been successfully employed in the treatment of alcoholism by Prof. Beaunis. —*Revue Philosophique*, July 1885.

Majesty's Inspectors of Schools, had with him when he visited Birmingham, a man and a boy, both of whom had been confirmed epileptics. They were hypnotised regularly every evening, and neither has had an attack since they commenced their tour with M. D'Anqnier. But some may ask, How does hypnotism affect this cure? When we know what epilepsy is we shall be in a position to answer that question. I do not suggest that hypnotism will cure epilepsy depending upon a depressed fracture of the skull, here the treatment consists in removing the depressed bone, but where there is no gross brain lesion and the seizures are due to some change in the blood vessels of the brain, we have in hypnotism a similar vascular change, which, in some way takes the place of the epileptic convulsions, and its employment is quite as rational as any of the recognised methods of treating epilepsy.

I need hardly dwell again upon its employment as an anæsthetic. We have seen that the gravest operations can be performed during hypnosis without the patient feeling any pain. There is a large number of cases on record but not a single instance of a death occurring, as unfortunately happens sometimes with every other anæsthetic. There are numerous cases of neuralgia, insomnia, functional paralysis, and intractable hysteria, being treated with success by this means, and recently several accouchments* have been conducted under hypnosis.

There is nothing more difficult to treat than hypochondriasis. Surely, in these cases, hypnotism should be tried before these wretched sufferers are permitted to waste their lives in going the round of the doctors, ultimately to end their days in an asylum.

I have employed hypnotism myself in the treatment of sick headache, hysteria, functional paralysis, neuralgia, and neurasthenia, with good effect. I have tried it, too, in chorea, but have seen no benefit from it.

In endeavouring to form an idea of its real value as a therapeutic agent, we must remember that what good has already come of it has come in spite of the opposition of almost the whole of the medical profession, and that any

* Archives de Tocologie des Maladies des Femmes—January, 1888.

medical man, who has taken this subject up with the view of finding out the truth, has placed himself in danger of being boycotted as a charlatan by the members of his own profession.

It fell into disrepute, partly because its early advocates pretended that the phenomena were due to some force which they generated, which was only in the power of few to generate; partly on account of the facilities it afforded knaves to practice deceit upon the investigator, for their own advantage. But with our modern methods, fraud on the part of the subject is at once detected.

Another reason why it has never been generally adopted is that only a small percentage of persons can be hypnotized. The idea of preparing the patient previously by a course of treatment has only recently been suggested; to what extent it will remove this objection, we are not yet in a position to say.

There are many dangers connected with hypnotism. Some persons, instead of passing into a state of legarthy, are seized with epileptic convulsions. I have seen a person in these convulsions on the platform of a professional mesmerist at a public entertainment. It has been proved that persons can make no resistance under hypnosis. There is a case on record, of a lady in Paris being hypnotized by a beggar in the street, who caused her to follow him to his home, and then robbed and assaulted her.

Persons can be made to sign their names, without knowing it, under hypnosis, and to commit crimes when they come round.

No one really knows the amount of harm caused by public exhibitions of mesmerism. Two persons were accidentally discovered, in a public hall in Birmingham, after an exhibition of mesmerism, in a semi-conscious state.

Then, too, there are many rumours of persons having lost their reason by being too frequently mesmerised. This, however, is no objection to its legitimate employment, for frequent inhalation of ether, intemperance in alcohol, absinthe, opium, and excesses of many kinds are recognized causes of insanity.

These abuses constitute a grave and urgent necessity for legislation upon this matter.

All these dangers would be avoided by making it illegal for anyone to hypnotize who does not possess a medical qualification.

Hypnotism should be looked upon in the light of administering an anæsthetic. No sane being would go upon a public platform and permit an unskilled person to administer chloroform to him, for the sake of amusing others. The physician, too, should look at it in the same light, and only employ it under the same circumstances, and with similar precautions.

Concerning some of the more dangerous experiments; we must remember we are dealing with what is practically a new remedy. When a new drug is discovered, experiments are always necessary to determine its dangers and its dose. These experiments are necessary for its scientific use without risk, and when once recorded need never be repeated.

The application of hypnotism to the study of the mind has opened an entirely new field for scientific labour, and has removed psychology from the cloudy region of metaphysics to the *terra firma* of an experimental science.

The modern application of hypnotism to the treatment of disease appears to be the necessary and rational outcome of that most important discovery of modern physiology—that the brain exerts a direct influence over the nutrition of the body.

Lord Bacon conceived the possibility of such an agent being found when he wrote:—"It is an enquiry of great depth and worth, concerning the imagination, how, and how far it altereth the body proper of the imaginant." "If the imagination fortified have power, then it is material to know how to fortify and exalt it."

It is impossible yet for any one to speak dogmatically concerning the value of this medicine for the mind. I have endeavoured to treat the subject impartially, and to elicit the truth; for while we remain in our present state of ignorance upon this subject we live in a constant state of danger, of another Anthony Mesmer arising, and a wave of superstition passing over the country, and sweeping away the feeble bulwarks against error, erected so patiently in recent times.

BIRMINGHAM :

HUDSON AND SON, PRINTERS, EDMUND STREET.

BIRMINGHAM AND MIDLAND INSTITUTE.

The Life Work of a Chemist:

AN ADDRESS

DELIVERED IN THE TOWN HALL, BIRMINGHAM,

ON THE 7TH OCTOBER, 1889,

SIR HENRY E. ROSCOE, M.P., D.C.L., LL.D., F.R.S.,

PRESIDENT.

Birmingham:

PRINTED BY DAVIS BROTHERS, LIVERY STREET

AND LIONEL STREET.

THE LIFE WORK OF A CHEMIST.

In asking myself what subject I could bring before you on the present occasion, I thought I could not do better than point out by one example what a chemist may do for mankind. And in choosing this theme for my discourse I found myself in no want of material, for amongst the various aspects of scientific activity there is surely none which, whether in its most recondite forms or in those most easily understood, have done more to benefit humanity than those which have their origin in my own special study of chemistry. I desired to show what one chemist may accomplish, a man devoted heart and soul to the investigation of nature, a type of the ideal man of science—whose example may stimulate even the feeblest amongst us to walk in his footsteps if only for a short distance, whose life is a consistent endeavour to seek after truth if haply he may find it, whose watchwords are simplicity, faithfulness, and industry, and whose sole ambition is to succeed in widening the pathway of knowledge so that following generations of wayfarers may find their journies lightened and their dangers lessened.

Such men are not uncommon amongst the ranks of distinguished chemists. I might have chosen as an example the life and labours of your sometime townsman, Joseph Priestley, had not this theme been already treated by Professor Huxley, in a manner I cannot approach, on the occasion of the inauguration of the statue which stands hard by. To-day, however, I will select another name, that of a man still living, the great French chemist—Pasteur.

As a chemist Pasteur began life, as a chemist he is ending it. For although, as I shall hope to point out, his most important researches have entered upon fields hitherto tilled, with but scanty success, by the Biologist, yet in his hands, by the application of chemical methods, they have yielded a most bountiful harvest of new facts of essential service to the well-being and progress of the human race.

And after all the first and obvious endeavour of every cultivator of science ought to be to render service of this kind. For although it is foolish and shortsighted to decry the pursuit of any form of scientific study because it may be as yet far removed from practical application to the wants of man, and although such studies may be of great value as an incentive to intellectual activity, yet the statement is so evident as to almost amount to a truism that discoveries which give us the power of rescuing a population from starvation, or which tend to diminish the ills that flesh, whether of man or beast, is heir to, must deservedly attract more attention and create a more general interest than others having so far no direct bearing on the welfare of the race.

"There is no greater charm," says Pasteur himself, "for the investigator than to make new discoveries, but his pleasure is more than doubled when he sees that they find direct application in practical life." To make discoveries capable of such an application has been the good fortune—by which I mean the just reward—of Pasteur. How he made them is the lesson which I desire this evening to teach. I wish to show that these discoveries, culminating as the latest and perhaps the most remarkable of all, in that of a cure for the dreaded and most fearful of all fearful maladies, hydrophobia, have not been, in the words of Priestley, "lucky haphazardings," but the outcome of patient and long-continued investigation. This latest result is, as I shall prove to you, not an isolated case of a happy chance, but simply the last link in a long chain of discoveries, each one of which has followed the other in logical sequence, each one bound to the other by ties which exhibit the life-work of the discoverer as one consequent whole. In order, however, to understand the end we must begin at the beginning, and ask ourselves what was the nature of the training of hand, eye, and brain which enabled Pasteur to wrest from Nature secret processes of disease the discovery of which had hitherto baffled all the efforts of biologists. What was the power by virtue of which he succeeded when all others had failed, how was he able to trace the causes and point out remedies for the hitherto unaccountable changes and sicknesses which beer and wine undergo? What means did he adopt to cure the fatal silkworm disease, the existence of which in the South of France in one year cost that country more than one hundred millions of francs? Or how did he arrive at a method for exterminating a plague known as fowl cholera, or that of the deadly cattle disease, anthrax, or splenic fever, which has killed millions of cattle, and is the fatal woolsorter's disease in man: and last but not least how did he

gain an insight into the working of that most mysterious of all poisons, the virus of hydrophobia?

To do more than point out the spirit which has guided Pasteur in all his work, and to give an idea of the nature of that work in a few examples, I cannot attempt in the time at my disposal. Of the magnitude and far reaching character of that work we may form a notion, when we remember that it is to Pasteur that we owe the foundation of the science of Bacteriology, a science treating of the ways and means of those minute organisms called microbes, upon whose behaviour the very life not only of the animal, but perhaps also of the vegetable world depends; a science which bids fair to revolutionise both the theory and practice of Medicine, a science which has already in the hands of Sir Joseph Lister given rise to a new and beneficent application in the discovery of antiseptic surgery.

The whole secret of Pasteur's success may be summed up in a few words. It consisted in the application of the exact methods of physical and chemical research to problems which had hitherto been attacked by other less precise and less systematic methods. His early researches were of a purely chemical nature. It is now forty years ago since he published his first investigation. But this pointed out the character of the man, and indicated the lines upon which all his subsequent work was laid.

Of all the marvellous and far-reaching discoveries of modern chemistry, perhaps the most interesting and important is that of the existence of compounds which—whilst possessing an identical composition, that is, made up of the same elements in the same proportions—are absolutely different substances judged of by their properties. The first instance made known to us of such isomeric bodies, as they are termed by the chemist, was that pointed out by the great Swedish chemist, Berzelius. He showed that the tartaric acid of wine-lees possesses precisely the same composition as a rare acid having quite different properties and occasionally found in the tartar deposited from wine grown in certain districts in the Vosges. Berzelius simply noted this singular fact, and did not attempt to explain it. Later on, Biot observed that not only do these two acids differ in their chemical behaviour, but likewise in their physical properties, inasmuch as the one (the common acid) possessed the power of deviating the plane of a polarised ray of light to the right, whereas the rare acid has no such rotatory power. It was reserved, however, for Pasteur to give the explanation of this singular and at that time unique phenomenon, for he proved that the optically inactive acid is made up of two compounds, each possessing the same composition, but differing in optical properties.

The one turned out to be the ordinary dextro-rotatory tartaric acid; the other a new acid which rotates the plane of polarisation to the left to an equal degree. As indicating the germ of his subsequent researches, it is interesting here to note that Pasteur proved that these two acids can be separated from one another by a process of fermentation, started by a mere trace of a special form of mould. The common acid is thus first decomposed, so that if the process be carried on for a certain time, only the rarer laevo-rotatory acid remains.

Investigations on the connection between crystalline form, chemical composition, and optical properties, occupied Pasteur for the next seven years, and their results—which seem simple enough when viewed from the vantage ground of accomplished fact—were attainable solely by dint of self-sacrificing labours such as only perhaps those who have themselves walked in these enticing and yet often bewildering paths can fully appreciate, and by attention to minute detail as well as to broad principles to an extent which none can surpass and few can equal. A knowledge of the action of the mould in the changes it effects on tartaric acid led Pasteur to investigate that *bête noir* of chemists, the process of fermentation. The researches thus inaugurated in 1857 not only threw a new and vivid light on these most complicated of chemical changes and pointed the way to scientific improvements in brewing and wine-making of the greatest practical value, but were the stepping-stones to those higher generalisations which lie at the foundation of the science of Bacteriology, carrying in their train the revolutions in modern medicine and surgery to which I have referred.

The history of the various theories from early times until our own day which have been proposed to account for the fact of the change of sugar into alcohol, or that of alcohol into vinegar, under certain conditions, a fact known to the oldest and even the most uncivilized of races, is one of the most interesting chapters in the whole range of chemical literature, but, however enticing, is one into which I cannot now enter. Suffice it here to say that it was Pasteur who brought light out of darkness by explaining conflicting facts and by over-turning false hypotheses. And this was done by careful experiment and by bringing to bear on the subject an intelligence trained in exact methods and in unerring observation, coupled with the employment of the microscope and the other aids of modern research.

What now did Pasteur accomplish? In the first place he proved that the changes occurring in each of the various processes of fermentation are due to the presence and growth of a minute organism called the ferment. Exclude all trace of these ferments, and no

change occurs. Brewers' wort thus preserved remains for years unaltered. Milk and other complex liquids do not turn sour even on exposure to pure air, provided these infinitely small organisms are excluded. But introduce even the smallest trace of these microscopic beings, and the peculiar changes which they alone can bring about at once begin. A few cells of the yeast plant set up the vinous fermentation in a sugar solution. This is clearly stated by Pasteur as follows:—"My decided opinion," he says, "on the nature of alcoholic fermentation is the following.—The chemical act of fermentation is essentially a correlative phenomenon of a vital act beginning and ending with it. I think that there is never any alcoholic fermentation without there being at the same time organisation development, multiplication of globules, or the continued consecutive life of globules already formed."

Add on a needle's point a trace of the peculiar growth which accompanies the acetous fermentation, and the sound beer or wine in a short time becomes vinegar. Place ever so small a quantity of the organism of the lactic fermentation in your sweet milk, which may have been preserved fresh for years in absence of such organisms, and your milk turns sour. But still more, the organism (yeast) which brings about the alcoholic fermentation will not give rise to the acetous, and *vice versâ*, so that each peculiar chemical change is brought about by the vital action of a peculiar organism. In its absence the change cannot occur; in its presence only that change can take place.

Here again we may ask, as Pasteur did—Why does beer or wine become sour when exposed to ordinary air? And the answer to this question was given by him in no uncertain tone in one of the most remarkable and most important of modern experimental researches. Milk and beer which have become sour on standing in the air contain living micro-organisms which did not exist in the original sound fluids. Where did these organisms originate? Are they or their germs contained in the air, or are these minute beings formed by a process of spontaneous generation from material not endowed with life?

A controversy as to the truth or falsity of the theory of spontaneous generation was waged with spirit on both sides, but in the end Pasteur came off victorious, for by a series of the most delicate and convincing of experiments he proved the existence of micro-organic forms and their spores—or seeds—in the air, and showed that whilst unpurified air was capable of setting up fermentative changes of various kinds, the same air freed from germs could not give rise to these changes. Keep away the special

germ which is the incentive to the pathological change, and that change cannot occur. In the interior of the grape, in the healthy blood, no such organisms, no such germs exist; puncture the grape or wound the animal body, and the germs floating in the air settle on the grape-juice or on the wounded tissue, and the processes of change, whether fermentative or putrefactive, set in with all their attendant symptoms. But crush the grape or wound the animal under conditions which either preclude the presence or destroy the life of the floating germ, and again no such change occurs; the grape-juice remains sweet, the wound clean.

I have said that every peculiar fermentative change is accompanied by the presence of a special ferment. This most important conclusion has only been arrived at as the result of careful experimental enquiry. How was this effected? By the artificial cultivation of these organisms. Just as the botanist or gardener picks out from a multitude of wild plants the special one which he wishes to propagate, and planting it in ground favourable to its growth, obtains fresh crops of the special plant he has chosen, so the Bacteriologist can by a careful process of selection obtain what is termed a pure cultivation of any desired organism. Having obtained such a pure cultivation, the next step is to ascertain what are the distinctive properties of that special organism; what characteristic changes does it bring about in material suitable for its growth. This having been determined, and a foundation for the science having thus been laid, it is not difficult to apply these principles to practice, and the first application made by Pasteur was to the study of the diseases of beer and wine.

In September, 1871, Pasteur visited one of the large London breweries, in which the use of the microscope was then unknown. A single glance at the condition of the yeast instantly told its tale, and enabled him to explain to the brewers the cause of the serious state of things, by which frequently as much as 20 per cent of their product was returned on their hands as unsaleable—this being that this yeast contained foreign or unhealthy organisms. And just as pure yeast is the cause of the necessary conversion of wort into beer, so these strange forms which differ morphologically from yeast, and whose presence can therefore be distinctly ascertained, are the cause of acidity, ropiness, turbidity, and other diseases which render the beer undrinkable. It is no exaggeration to say that, whereas, before Pasteur's researches the miscroscope was practically unknown in the brewhouse, it has now become as common as the thermometer or the saccharometer, and by its help and by the interpretations we can place upon its revelations through

Pasteur's teaching, yeast—of all brewers' materials the least open to rough and ready practical discernment—becomes easy of valuation as to its purity or impurity, its vigour or weakness, and, therefore, its behaviour during fermentation. Thus while in former days the most costly materials were ever liable to be ruined by disease organisms unconsciously introduced into them with the yeast, at the present day the possibilities of any such vast pecuniary disasters become easily avertable.

Of all industries, brewing is perhaps the one which demands the most stringent care in regard to complete and absolute cleanliness. The brewers' materials, products, and bye-products, are so putrescible; there is always so vast an abundance of disease organisms in the brewery air, that the minutest amounts of these waste products lying about in vessels or pipes transform these places into perfect nests for the propagation of these micro-organisms, whence, transferred into the brewings, they inevitably ruin them however carefully and scientifically prepared in other respects. Without the microscope, any breach of discipline in the way of the supreme cleanliness necessary is impossible of detection; with it we can track down the micro-organisms to their source, whether it be in uncleanly plant, in impurity of materials, or in carelessness of manipulation.

Among the more direct applications of Pasteur's researches, the so-called Pasteurization of beer claims a place. Pasteur showed that temperatures well below the boiling point sufficed for destroying the disease organisms in alcoholic fluids, and based on these results, enormous quantities of low-fermentation beers are annually submitted to these temperatures, and thus escape the changes otherwise incident to the micro-organisms which have succumbed to the treatment. This process is however, for several intricate reasons, not suited for English beers, but if we cannot keep our beers by submitting them to high temperatures, we can foretell to a nicety how they will keep by artificially forcing on those changes, which would occur more slowly during storage. The application of a suitable temperature, the exclusion of outside contamination, a microscopic examination of the "forced" beer, and the knowledge which we owe to Pasteur of what the microscopic aspects means, suffice to make each brewing foretell its own future history, and thus suffice to avert the otherwise inevitable risks incident to the storage and export of beer, the stability of which is unknown.

Brewing has thus become a series of precise and definite operations, capable of control at every point. Instead of depending —as it had to depend—on intuition and experience handed down in secrecy from father to son, it now depends upon care, fore-

thought, and the soundness of the brewer's scientific training. This change in the nature of the brewer's operations, and in the persons who govern them, is primarily due to Pasteur. Other men have done much to carry on his work, but it is to his example of ceaseless patience, and to his example of freely publishing to the world all the results of his work, that the brewers of all countries are indebted for the connection of each phenomenon with a controllable cause, and for thus emancipating their industry from empiricism and quackery.

Much the same story has to be told about Pasteur's investigation of wine and its diseases. As with the brewer, so with the wine-grower Pasteur has pointed out the causes of his troubles, and the causes having been ascertained, the remedies soon followed, and the practical value of these researches to the trade of France and other wine-producing countries has been enormous.

The next labour of our scientific Hercules was of a different kind, but of a no less interesting or important a character. The South of France is a great silk-producing district. In 1853 the value of the raw silk was represented by a sum of some five millions sterling, and up to that date the revenue from this source had been greatly augmenting. Suddenly this tide of prosperity turned, a terrible plague broke out amongst the silk-worms, and in 1865 so general had the disease become that the total production of French silk did not reach one million, and the consequent poverty and suffering endured in these provinces became appalling. Every conceviable means was tried to overcome the disease, but all in vain. The population and the government of France—for the evil was a national one—were at their wits end, and a complete collapse of one of the most important French industries seemed inevitable. Under these circumstances the great chemist Dumas, who was born at Alais, in the centre of one of the districts most seriously affected, urged his friend Pasteur to undertake an investigation of the subject. Pasteur, who at this time had never seen a silk-worm, naturally felt diffident about attempting so difficult a task, but at last, at Dumas' renewed entreaty, he consented, and in June, 1865, betook himself to the south for the purpose of studying the disease on the spot. His previous training here again stood him in good stead, and in September, 1865, he was able to communicate to the Academy of Sciences results of observation and experiment which, striking at the root of the evil, pointed the way to the means of securing immunity from the dreaded plague. This paper was freely criticised. Here, it was said, was a chemist who, quitting his proper sphere, had the hardihood to lay down rules for

the guidance of the physician and biologist in fields specially their own. Why should his proposals be more successful than all the other nostrums which had already so egregiously failed?

In order to appreciate the difficulties which met Pasteur in this enquiry, and to understand how wonderfully he overcame them, I must very shortly describe the nature of this disease, which is termed Pébrine, from the black spots which cover the silk-worm. It declares itself by the stunted and unequal growth of the worms, by their torpidity, and by their fastidiousness as to food, and by their premature death.

Before Pasteur went to Alais the presence of certain microscopic corpuscles had been noticed in the blood and in all the tissues of the diseased caterpillar, and even in the eggs from which such worms were hatched. These micro-organisms often fill the whole of the silk organs of the insect, which in a healthy condition contain the clear viscous liquid from which the silk is made. Such worms are of course valueless. Still this knowledge did not suffice, for eggs apparently healthy gave rise to stricken worms incapable of producing silk, whilst again other worms distinctly diseased yielded normal cocoons. These difficulties, which had proved too much for previous observers, were fully explained by Pasteur. "The germs of these organisms," said he, "which are so minute, may be present in the egg and even in the young worms, and yet baffle the most careful search. They develop with the growth of the worm, and in the chrysalis they are more easily seen. The moth derived from a diseased worm invariably contains these corpuscles, and is incapable of breeding healthy progeny."

This moth-test is the one adopted by Pasteur, and it is an infallible one. If the female moth is stricken, then her eggs—even though they show no visible sign of disease—will produce sick worms. If in the moth no micro-cocci are seen, then her immediate progeny at any rate will be sound and free from inherited taint, and will always produce the normal quantity of silk. But this is not all. Pasteur found that healthy worms can be readily infected by contact with diseased ones, or through germs contained in the dust of the rooms in which the worms are fed. Worms thus infected, but free from inherited taint, can however, as stated, spin normal cocoons, but—and this is the important point—the moths which such chrysalides yield invariably produce diseased eggs. This explains the anomalies previously noticed. The silk-worms which die without spinning are those in which the disease is hereditary, viz., those born from a

diseased mother. Worms from sound eggs which contract the disease during their life-time always spin their silk, but they give rise to a stricken moth, the worms from which do not reach maturity and furnish no silk.

As I have said, these results were but coldly received. It was hard to make those engaged in rearing the worms believe in the efficacy of the proposed cure. Then, seeing this state of things, Pasteur determined to take upon himself the *rôle* of a prophet. Having in 1866 carefully examined a considerable number of the moths which had laid eggs intended for incubation, he wrote down a prediction of what would happen in the following year with respect to the worms hatched from these eggs. In due course, after the worms from a mixed batch of healthy and unhealthy eggs had spun, the sealed letter was opened and read, and the prediction compared with the actual result, when it was found that in 12 out of 14 cases there was absolute conformity between the prediction and the observation, for 12 hatchings were predicted to turn out diseased, and this proved to be the case. Now all these "educations" were believed to be healthy by the cultivators, but Pasteur foretold that they would turn out to be diseased by the application of the moth-test in the previous year. Two other parcels of eggs were pronounced by Pasteur to be sound, because they were laid by healthy moths containing none of the micro-cocci, and both these yielded a healthy crop. So successful a prophecy could not but gain the belief of the most obtuse of cultivators, and we are not surprised to learn that Pasteur's test was soon generally applied, and that the consequence has been a return of prosperity to districts in which thousands of homes had been desolated by a terrible scourge.

I must now ask you to accompany me to another and a new field of Pasteur's labours, which perhaps more than his others claims your sympathy and will enlist your admiration, because they have opened out to us the confident hope of at least obtaining an insight into some of the hidden causes and therefore to the possible prevention of disease.

In the first place I must recall to your remembrance that most infectious diseases seldom if ever recur, and that even a slight attack renders the subject of it proof against a second one. Hence inoculation from a mild case of small-pox was for a time practiced, but this too often brought about a serious if not fatal attack of the malady, and the step taken by Jenner of vaccinating, that is of replacing for the serious disease a slight one which

nevertheless is sufficient protection against small-pox infection, was one of the highest importance. But Jenner's great discovery has up to recent years remained an isolated one, for it led to no general method for the preventative treatment of other maladies, nor had any explanation been offered of its mode of action. It is to Pasteur that science is indebted for the generalization of Jenner's method, and for an explanation which bids fair to render possible the preventative treatment of many—if not of all—infectious diseases. It was his experience based upon his researches on fermentation that led to a knowledge of the nature of the poison of such diseases, and showed the possibility of so attenuating or weakening the virus as to furnish a general method of protective or preventive inoculation.

I have already pointed out how a pure cultivation of a microbe can be effected. Just as the production of pure alcohol depends on the presence of the pure yeast, so special diseases are dependent on the presence of certain definite organisms which can be artificially cultivated, and which give rise to the special malady. Can we now by any system of artificial cultivation so modify or weaken the virus of a given microbe as to render it possible to inoculate a modified virus which, whilst it is without danger to life, is still capable of acting as a preventive to further attack? This is the question which Pasteur set himself to solve, nor was the task by any means an apparently hopeless one. He had not only the case of Jennerian vaccination before him, but also the well-known modifications which cultivation can bring about in plants. The first instance in which Pasteur succeeded in effecting this weakening of the poison, was in that of a fatal disease to which poultry in France are very liable, called chicken cholera. Like many other maladies, this is caused by the presence of a micro-organism found in the blood and tissues of the stricken fowl. One drop of this blood brought under the skin of a healthy chicken kills it, and the same microbe is found throughout its body. And if a pure culture of these microbes be made, that culture—even after a series of generations—is as deadly a poison as the original blood. Now comes the discovery. If these cultures be kept at a suitable temperature for some weeks exposed to pure air, and the poisonous properties tested from time to time, the poison is found gradually to become less powerful, so that after the lapse of two months a dose which had formerly proved fatal now does not disturb in the slightest the apparent health of the fowl. But now let us inoculate a chicken with this weakened virus. It suffers a slight illness, but soon recovers. Next let us

give it a dose of the undiluted poison, and, as a control, let us try the action of the same on an unprotected bird. What is the result? Why that the first chicken remains unaffected, whilst the second bird dies. The inoculation has rendered it exempt from the disease, and this has been proved by Pasteur to be true in thousands of cases, so that whereas the death-rate in certain districts amongst fowls before the adoption of Pasteur's inoculation method was 10 per cent, after its general adoption it has diminished to less than 1 per cent.

We can scarcely value too highly this discovery, for it proves that the poisonous nature of the microbe is not unalterable, but that it can be artificially modified and reduced, and thus an explanation is given of the fact that in an epidemic the virus may either be preserved or become exhausted according to the conditions to which it is subjected. We have here to do with a case similar to that of Jenner's vaccine, except that here the relation between the weak and the strong poison has become known to us, whilst in Jenner's case it has lain concealed. This then is the first triumph of experimental enquiry into the cause and prevention of microbic disease, and this method of attenuation is of great importance, because, as we shall see, it is not confined to the case of chicken cholera, but is applicable to other diseases.

And next I will speak of one which is a fatal scourge to cattle, and is not unfrequently transmitted to man. It is called anthrax, splenic fever, or woolsorters' disease. This plague, which has proved fatal to millions of cattle, is also due to a microbe, which can be cultivated like the rest, and the virus of which can also be weakened or attenuated by a distinct treatment which I will not here further specify. Now, what is the effect of inoculating cattle or sheep with this weakened poison? Does it act as a preventative? That the answer is in the affirmative was proved by Pasteur by a convincing experiment. Five-and-twenty sheep, chosen promiscuously out of a flock of fifty, were thus inoculated with the weak virus, then after a time all the fifty were treated with the strong poison. The first half remained healthy, all the others died of anthrax. Since the discovery of this method, no fewer than 1,700,000 sheep and about 90,000 oxen have thus been inoculated, and last year 269,599 sheep and 34,464 oxen were treated. The mortality which, before the introduction of the preventative treatment, was in the case of sheep ten per cent, was after the adoption of the method reduced to less than one per cent. So that now the farmers in the stricken districts have all adopted the process, and agricultural insurance societies make the preventative inoculation a *sinè quâ*

non for insuring cattle in those districts. This is however not the end of this part of my story, for Pasteur can not only thus render the anthrax poison harmless, but he has taught us how to bring the highly virulent poison back again from the harmless form. This may go to explain the varying strength of an attack of infectious disease, one case being severe and another but slight, due to the weakening or otherwise of the virus of the active microbe.

Last, but not least, I must refer to the most remarkable of all Pasteur's researches, that on Rabies and Hydrophobia. Previous to the year 1880, when Pasteur began his study of this disease, next to nothing was known about its nature. It was invested with the mysterious horror which often accompanies the working of secret poisons, and the horror was rendered greater owing to the fact that the development of the poison brought in by the bite or by the lick of a mad dog might be deferred for months, and that if after that length of time the symptoms once make their appearance, a painful death was inevitable. We knew indeed that the virus was contained in the dog's saliva, but experiments made upon the inoculation of the saliva had led to no definite results, and we were entirely in the dark as to the action of the poison until Pasteur's investigation. To begin with he came to the conclusion that the disease was one connected with the nerves, and to the nerve centres he therefore looked as the seat of the virus or of the microbe. And he proved by experiment that this is the case, for a portion of the matter of the spinal column of a rabid dog, when injected into a healthy one, causes rabies with a much greater degree of certainty and rapidity than does the injection of the saliva. Here then we have one step in advance. The disease is one of the nerves, it only exhibits itself when the nerve centres are attacked. And this goes to explain the varying times of incubation which the attack exhibits. The virus has to travel up the spinal cord before the symptoms can manifest themselves, and the length of time taken over that journey depends on many circumstances. If this be so, the period of incubation must be lessened if the virus is at once introduced into the nerve centres. This was also proved to be the case, for dogs inoculated under the *dura mater* invariably became rabid within a period rarely exceeding eighteen days.

Next came the question: can this virus be weakened as has been proved possible with the former poisons? The difficulty in this case was greater, inasmuch as all attempts to isolate or to cultivate the special microbe of rabies outside the animal body had failed. But Pasteur's energy and foresight overcame this difficulty, and a

method was discovered by which this terrible poison can so far be weakened as to lose its virulent character, but yet remain potent enough, like the cases already quoted, to act as a preventive; and dogs which had thus been inoculated were proved to be so perfectly protected, that they might be bitten with impunity by mad dogs, or inoculated harmlessly with the most powerful rabic virus.

But yet another step. Would the preventive action of the weakened virus hold good when it is inoculated even after the bite? If so, it might be thus possible to save the lives of persons bitten by mad dogs. Well, experiment has also proved this to be true, for a number of dogs were bitten by mad ones, or were inoculated under the skin with rabic virus; of these some were subjected to the preventive cure and others not thus treated. Of the first or protected series not one became mad, of the other or unprotected dogs a large number died with all the characteristic symptoms of the disease. But it was one thing to thus experiment upon dogs, and quite another thing, as you may well imagine, to subject human beings to so novel and perhaps dangerous a treatment. Nevertheless, Pasteur was bold enough to take this necessary step, and by so doing has earned the gratitude of the human race.

In front of the Pasteur Institute in Paris stands a statue worked in consummate skill in bronze. It represents a French shepherd boy engaged in a death struggle with a mad dog which had been worrying his sheep. With his bare hands, and with no weapon save his wooden *sabot*, the boy was successful in the combat. He killed the dog, but was horribly bitten in the fight. The group represents no mythical struggle, the actual event took place in October, 1885; and this boy, Jupille, was the second person to undergo the anti-rabic treatment, which proved perfectly successful, for he remained perfectly healthy, and his heroic deed and its consequences have become historic. "*C'est le premier pas qui coute*," and as soon as the first man had been thus successfully treated, others similarly situated gladly availed themselves of Pasteur's generous offer to treat them gratuitously. And as soon as this cure became generally known, crowds of persons of all ages, stations, and countries, all bitten by rabid animals, visited every day Pasteur's laboratory in the Rue d'Ulm, which, from being one in which quiet scientific researches was carried on, came to resemble the out-patient department of a great hospital. There I saw the French peasant, the Russian moujik (suffering from the terrible bites of rabid wolves), the swarthy Arab, the English policeman, with women

too and children of every age, in all perhaps a hundred patients. All were there undergoing the careful and kindly treatment, which was to ensure them against a horrible death. Such a sight will not be easily forgotten. By degrees this wonderful cure for so deadly a disease attracted the attention of men of science throughout the civilised world. The French nation raised a monument to the discoverer better than any statue, in the shape of the "Pasteur Institute"— An institution devoted to carrying out in practice this anti-rabic treatment, with laboratories and every other convenience for extending by research our knowledge of the preventive treatment of infectious disease.

For be it remembered, we are only at the beginning of these things, and what has been done is only an inkling of what is to come. Since 1885, twenty anti-rabic institutions have been established in various parts of the world, including Naples, Palermo, Odessa, St. Petersburg, Constantinople, Rio Janeiro, Buenos Ayres, and Havannah.

We in England have also taken our share, though a small one, in this work. In 1886 I moved in the House of Commons for a committee to investigate and report on Pasteur's anti-rabic method of treatment. This committee consisted of trusted and well-known English men of science and physicians— Sir James Paget, Sir Joseph Lister, Drs. Burdon Sanderson, Lauder Brunton, Quain, Fleming, and myself, with Prof. Victor Horsley as secretary. We examined the whole subject, investigated the details of a number of cases, repeated Pasteur's experiments on animals, discussed the published statistics, and arrived unanimously at the opinion that Pasteur was justified in his conclusions, and that his anti-rabic treatment had conferred a great and lasting benefit on mankind. Since then His Royal Highness the Prince of Wales, who always takes a vivid interest in questions affecting the well-being of the people, has visited the Pasteur Institute, and has expressed himself strongly in favour of a movement, started by the present Lord Mayor of London, for showing to Pasteur, by a substantial grant to his institute, our gratitude for what he has done to relieve upwards of 250 of our countrymen who have undergone treatment at his hands, and likewise to enable poor persons who have been bitten, to undertake the journey to Paris, and the sojourn there necessary for their treatment. This lasts about a fortnight, it is nearly painless, and no single case of illness, much less of hydrophobia—due to the preventative treatment—has occurred amongst the 7,000 persons who have so far undergone the cure.

Now let me put before you the answer to the question: is this treatment a real cure? For this has been doubted by persons,

some of whom will I fear still doubt or profess to doubt, and still abuse Pasteur whatever is said or done! From all that can be learnt about the matter, it appears pretty certain that about from fifteen to twenty persons out of every hundred bitten by mad dogs or cats, and not treated by Pasteur's method, develop the disease, for I need scarcely add that all other methods of treatment have proved fallacious; but bites on the face are much more dangerous, the proportion of fatal cases reaching eighty per cent. Now of 2,164 persons treated in the Pasteur Institute, from November, 1885, to January, 1887, only thirty-two died, showing a mortality of 1·4 per cent instead of fifteen to twenty, and amongst these upwards of 2,000 persons, 214 had been bitten on the face, a class of wounds in which, as I have said, when untreated, the mortality is very high; so that the reduction in the death-rate seems more remarkable, especially when we learn that in all these cases the animal inflicting the wound had been proved to be rabid. The same thing occurs even in a more marked degree in 1887 and 1888. In 1887, 1,778 cases were treated with a mortality of 1·3 per cent, whilst last year 1,626 cases were treated with a mortality of 1·16 per cent.

Statistics of the anti-rabic treatment in other countries show similar results, proving beyond a doubt that the death rate from hydrophobia is greatly reduced. Indeed, it may truly be said that in no case of dangerous disease, treated either by medicine or surgery, is a cure so probable. Moreover, in spite of assertions to the contrary, no proof can be given that in any single case did death arise from the treatment itself. And as showing the safety of the inoculation, I may add that all Pasteur's assistants and laboratory workers have undergone the treatment, and no case of hydrophobia has occurred amongst them.

You are no doubt aware that Pasteur's anti-rabic treatment has been strongly opposed by certain persons, some of whom have not scrupled to descend to personal abuse of a virulent character of those who in any way encouraged or supported Pasteur's views, and all of whom persistently deny that anything good has come or can come from investigations of the kind. Such persons we need neither fear nor hate. Their opposition is as powerless to arrest the march of science as was King Canute's order to stop the rising tide. Only let us rest upon the sure basis of exactly-ascertained fact, and we may safely defy alike the vapourings of the sentimentalist and the wrath of the opponent of scientific progress. But opposition of a much fairer character has likewise to be met, and it has with propriety been asked—how comes it

that Pasteur is not uniformly successful? Why, if what you tell us is true, do any deaths at all follow the anti-rabic treatment? The answer is not far to seek. Pasteur's treatment is really a race between a strong and an attenuated virus. In cases in which the bite occurs near a nerve centre, the fatal malady may outstrip the treatment in this race between life and death. If the weakened virus can act in time, it means life. If the strong virus acts first, prevention comes too late—it means death. So that the treatment is not doubtful in all cases, but only doubtful in those which are under well known unfavourable conditions. This, it seems to me, is a complete reply to those who ignorantly fancy that because Pasteur's treatment has not cured every case, it must be unreliable and worthless.

One word more. I have said that Pasteur is still, as he has always been—a chemist. How does this fit in with the fact that his recent researches seem to be entirely of a biological character? This is true. They seem, but they really are not. Let me in a few sentences explain what I mean. You know that yeast produces a peculiar chemical substance—alcohol. How it does so we cannot yet explain, but the fact remains. Gradually, through Pasteur's researches, we are coming to understand that this is not an isolated case, but that the growth of every micro-organism is productive of some special chemical substance, and that the true pathogenic virus—or the poison causing the disease—is not the microbe itself, but the chemical compound which its growth creates. Here once more "to the solid ground of nature trusts the man that builds for aye," and it is only by experiment that these things can be learnt.

Let me illustrate this by the most recent and perhaps the most striking example we know of. The disease of diptheria is accompanied by a peculiar microbe which, however, only grows outside, as it were, of the body, but death often takes place with frightful rapidity. This takes place not by any action of the microbe itself, but by simple poisoning due to the products of the growing organism, which penetrate into the system, although the microbe does not. This diptheritic *bacillus* can be cultivated, and the chemical poison which it produces can be completely separated by filtration from the microbe itself, just as alcohol can be separated from the yeast granules. If this be done, and one drop of this pelucid liquid given to an animal, that animal dies with all the well-known symptoms of the disease. This, and similar experiments made with the microbes of other diseases, lead to the conclusion that in infectious maladies the cause of death

is poisoning by a distinct chemical compound, the microbe being not only the means of spreading the infection, but also the manufacturer of the poison. But more than this, it has lately been proved that a small dose of these soluble chemical poisons confers immunity. If the poison be administered in such a manner as to avoid speedy poisoning, but so as gradually to accustom the animal to its presence, the creature becomes not only refractory to toxic doses of the poison, but also even to the microbe itself. So that instead of introducing the micro-organism itself into the body, it may now only be necessary to vaccinate with a chemical substance which in large doses brings about the disease, but in small ones confers immunity from it, reminding one of Hahnemann's dictum of "Similia similibus curantur."

Here then we are once more on chemical ground. True, on ground which is full of unexplained wonders, which, however, depend on laws we are at least in part acquainted with, so that we may in good heart undertake their investigation, and look forward to the time when knowledge will take the place of wonder.

In conclusion, I feel that some sort of apology is needed in thus bringing a rather serious piece of business before you on this occasion. Still I hope for your forgiveness, as my motive has been to explain to you as clearly as I could the life-work of a chemist who has in my opinion conferred benefits as yet untold and perhaps unexampled on mankind.